

Psychological Review

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THE PSYCHOLOGICAL REVIEW

THE STRUCTURING OF EVENTS: OUTLINE OF A GENERAL THEORY WITH APPLICATIONS TO PSYCHOLOGY¹

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I. THE PROBLEM OF STRUCTURE

It is generally assumed that the way to understand nature lies in the understanding of its laws. Some predictability and order in the objects studied are a prerequisite to knowledge about them. It is frequently assumed, also, that the laws of nature are essentially *quantitative*—that they express amounts of some measurable attribute and relationships by which one such variable is a function of another. Hull and his associates stated the matter as follows: "Since it appears probable that everything which exists at all in nature exists in some amount, it would seem that the ultimate form of all scientific postulates should be quantitative" (2, p. 8).

The purpose of the present article is to explore the possibility that, notwithstanding the ubiquity, precision, and unquestioned importance of quantitative and covariational formulas, there may be in nature *another type* of law that is quite as universal, objectively demonstrable, and, in its way, precise. The writer believes that this is true and that the knowledge of such a possible nonquantitative, but still fundamental, law (which, however, is neither "quali-

tative" nor "configurational") is as indispensable as quantitative formulations for a full understanding of any phenomenon, and that it is particularly needed at the present time in the field of psychology. It is to knowledge of this sort that we must turn for further illumination upon that still unsolved but vital problem, the organization of behavior. However useful they may be for descriptive purposes, the molar laws of covarying behavioral quantities have about reached the end of their tether so far as explanation is concerned. Some broader theoretical outlook is required if the treatment of these variables themselves is to acquire a deeper and more useful meaning. It is to the meeting of this theoretical need that the present article is addressed.

A word of admonition, however, should be said about its content. It must deal with issues that transcend psychology and pertain to all the sciences; for the problem of the nonquantitatively lawful in nature is universal. At first it might seem that the theory to be proposed, since it must be stated in correspondingly general terms, lies outside the scope of psychology proper. This impression would be erroneous. Though the reader may miss some of the familiar terminology, and though for want of space it will be necessary to ask him to make some of the applications for

¹ The writer gratefully acknowledges a generous grant of time by the authorities of Syracuse University for pursuing the theoretical and experimental studies upon which this article is based.

himself, the projected general model pertains at every turn to the task of explaining the fundamental processes of behavior. Among the problems upon which it specifically bears, within the limits of the present article, are the nature of psychological organization, motivation, learning, perception, and their interrelationship, the continuity-versus-discontinuity controversy, facilitation and inhibition, and the energies of attitudes. The writer believes that the theory is the *more* significant for psychology precisely *because* it has been developed in a wider frame of reference to meet a more universal challenge in science.

Let us begin by re-examining the role of quantitative statements in generalizations concerning behavior. The reader will find below a description of a familiar act or act sequence. The description is given wholly in terms of quantitative laws. Some of them are repeated as called for in the act, and nearly all are of the covariation type. The list is divided into five phases (A to E) to correspond to successive phases of the act sequence. Let us see how well the list describes and explains the phenomenon and whether we can identify the behavior involved.

A

1. The rate of evaporation varies with the temperature.

B

2. The curvature of the lens varies inversely with distance from the object of vision.
3. A neural impulse occurs at full intensity or not at all.
4. The magnitude of a neural impulse varies directly with the diameter of the neuron.
5. The intensity of a sensation increases directly with constant relative increments of the stimulus.
6. The more continuous the contour of an object the more readily it is perceived.

C

- (3.) A neural-transmission impulse occurs at full intensity or not at all.

- (4.) The magnitude of a neural impulse varies directly with the diameter of the neuron.

7. The energy of a muscle contraction varies directly with the number of muscle fibers excited.

Laws, 3, 4, and 7 now reappear in several repetitions, and, in connection with them, laws such as 2, 5, and 6.

D

8. The terminal velocity of a falling body is equal to the constant acceleration of gravity times the duration of the fall.

E

- (3.) A neural-transmission impulse occurs at full intensity or not at all.
- (4.) The magnitude of a neural impulse varies directly with the diameter of the neuron.
- (7.) The energy of a muscle contraction varies directly with the number of fibers excited.

There are repetitions of this series, interspersed with earlier laws.

- (8.) The terminal velocity of falling is equal to the constant acceleration of gravity times the duration of the fall.
9. The velocity of flowing varies inversely as the cross section of the flow.

If the reader now tries to state the behavior represented by the above list, he will probably be somewhat bewildered. He will guess that the phenomenon involves principles of gravitation and hydrodynamics along with behavior; but he cannot go much further than this with certainty. One wonders whether quantitative laws can be expected to *explain* an act sequence that they do not *describe* with sufficient completeness to permit its identification. Furthermore, there are important questions about the laws themselves that are unanswerable from the list: (a) Why are these *particular* laws brought together here, rather than countless others that could be mentioned? (b) How is their *order* in the list to be accounted for, including the repetitions indicated? (c) There appears to be very little "organization" of the laws in the list. Some organizing principle is

needed to help us understand what is happening. We look in vain for answers to these questions in the laws themselves. There is something essential that they fail to give.

Let us now redescribe the act sequence in other, more familiar, terms, using the same interspersed capital letters to indicate the various stages:

- (A) A small boy on a warm day, becoming thirsty,
- (B) sees a pitcher of lemonade and a glass on a buffet.
- (C) He goes to the dining room table, pulls out a chair, places it near the buffet, climbs upon it, pours lemonade
- (D) from the pitcher into the glass,
- (E) raises the glass . . . and drinks.

Now the matter is clear. By the use in this description of some terms *other than* quantitative we have been able to state the act sequence intelligibly. We are also able to answer, even on the basis of the meager knowledge of the events thus provided, all three of the questions *about the quantitative laws themselves* that these laws failed to answer. If we read the laws again, this time in conjunction with the acts listed above for the appropriate parts of the sequence, we can see the true basis of (a) the selection, (b) the ordering, and (c) the organization of the laws as they are "called into play." Something *other than quantities* of happenings has now been added to the picture, with a resulting increase of our understanding. The significance of the laws is clarified by showing that they are "contained," as it were, and ordered within a given self-delimited arrangement of happenings. Let us call this new "something" that is added to the quantitative laws in the form of a pattern of happenings the *structure* of the phenomenon in question. It will be seen that by this term we are *not* referring to anything that is "static." It is, rather, a *dynamic* structure—a structure of *events*.

Structure then, as thus defined, is what the quantitative laws fail to give; and it is what is needed (and needs more fully to be explained) if we are to have an adequate understanding of behavior. The structuring of events, to be sure, never occurs without the fact that the laws also hold good. Quantitative laws are demonstrable in all phenomena and are highly important and useful. Nevertheless our analysis shows that structure also is something that "holds good" and that must be considered in its own right. It seems possible that it may have laws of its own. If so, these laws will probably be of a different sort from the other (quantitative) laws; for we have seen that the latter do not describe the structure of phenomena and that the gaining of an inkling of the structure was necessary in order to answer certain questions about the quantitative laws themselves. We can go even further and say that if it were not for such a structure (comprising events of stimulus impingement, neural excitation, muscle-fiber activation, and the like), there would be no way of showing that the quantitative laws of behavior *exist*. The understanding of dynamic structure is therefore a matter of considerable importance.

Our statement of the laws in the example given was admittedly crude. Neither the definite equations nor specific quantities were given. If they had been stated more precisely and exemplified by quantities, would they then have been able to supply the necessary information and to identify the act sequence? It seems doubtful, since the added elements would still be abstractions so far as the actual pattern of events is concerned. It is true, also, that only a "sample" of the possible covariation laws was given. Suppose the list had been extended until it covered all the equations that could apply to a

boy's getting a drink of lemonade (no doubt a very great number). Could the problem of the pattern have been solved then? This, too, is doubtful. If the laws were rigorously limited to quantitative statements, the task might have been even more difficult than before. We shall probably have to conclude, then, that the failure of the quantitative laws to be fully descriptive and explanatory does not lie in the paucity of the laws available nor in a lack of precision in their statement. It lies, rather, in their inherent limitation with respect to the problem in hand.

It is true that the glimpse of structure that illuminated the second approach to our example had to be inferred from a molar account, which, like molar statements in general, provided very little of the actual detail of structurization. Still, it served to recall, from other experiences, what we did know about the structure of the organism's behavior, including its neurophysiological aspects in their relevance to the episode in question. The most pressing present problem for psychology, in the writer's opinion, is to pass from such crude molar descriptions to a closer analysis and delineation of the *structure* of behavioral acts. What is latent or implicit *behind* molar formulations that can give the illumination which quantitative laws fail to provide? We should not beg the question by saying that unless structure can be stated in quantitative terms it is unlawful and cannot be explained. The quantitative laws *themselves*, even "molar" laws, require some structural understanding. Nor can it be said that structure lacks generality. What could be more universal in behavior than the "general format" of eating or drinking, or of a hundred other behaviors sufficiently stable and recurrent to have been given a name? But the problem is broader, even, than the field of psychology.

Evidences of structures of a generalized sort occur in the phenomena of every science. If such structurings are general, and if they cannot be explained by quantitative laws, is it not logical to suppose that there may be such things as *structural* laws, that structure is something *sui generis*? There might even be some one universal structural principle that operates throughout the whole of nature.

The history of psychological schools and theories could, in a sense, be regarded as the record of attempts to deal with this problem in the field of psychology. They range from totalistic concepts such as *gestalten*, sign *gestalten*, supersummative wholes, cognitive maps, topological and brain fields, and "hypotheses," through open systems, mechanics of redintegration, communication and information, and mathematical brain models, to the stripped intervening constructs of behavior theory. Metaphysical postulates, also, such as "emergence," "entelechy," and various "manikin" assumptions have not been wanting. No final and conclusive answer has yet appeared. The very diversity of these efforts attests the difficulty of finding some clear, denotational way in which the structure of behavior can be described.

Perhaps the most general reason for the failure to solve the problem lies in the fact that it has not been approached in its own right. It has been assumed, in effect, that there is *no general* structural problem, that the means are already at hand for explaining each specific structure by the traditional methods of scientific logic. On the one hand it is assumed that, since every event must have some cause, the ordinary *logic of causality* should be able to explain the "structuring" of events. That we have not been able to explain matters this way is due merely to the fact

that we have not yet found the right cause. On the other hand, there is a tendency to believe that the *laws* which state covariations and thresholds of measurable quantities should be able to bring it about that each element of a phenomenon gets placed in the proper spatial position at the exact time and sequence required for its characteristic structuring. The fact that no one has been able to show how quantitative laws accomplish this feat has not been sufficiently taken to heart. It is forgotten that quantitative laws are merely descriptive statements, not causal agents or forces. Frequently a "mechanism" of some sort (a term borrowed without justification from mechanics) is "postulated," through which the laws are believed to "act," or within which they are said to be "manifested," and into which "intervening variables" can be injected to fill the gaps in our structural apprehension. The writer has discussed the assumptions underlying the belief that quantitative, or mechanical, laws are the "architects of structure" in a forthcoming work (1). We shall here turn our attention to the problem of "structural causality."

II. CAN STRUCTURE BE EXPLAINED BY "CAUSE AND EFFECT"?

According to the commonly accepted definition of cause and effect an event, O , is the "cause" of an event, P , if it precedes P and is a necessary and sufficient condition of P . If O then P ; if not O then not P . If we employ the symbol \supset to indicate contingency and a superimposed dot to show negation, this can be expressed as

$$\begin{aligned} O &\supset P \\ \dot{O} &\supset \dot{P}. \end{aligned}$$

Causes and effects can also be written as a linear series of single elements in

which each succeeding effect becomes a cause for the next effect, thus:

$$\begin{array}{c} M \supset N \supset O \supset P \\ \text{---etc.---} \rightarrow \quad \quad \quad \leftarrow \text{---etc.---} \\ \dot{M} \supset \dot{N} \supset \dot{O} \supset \dot{P} \\ \quad \quad \quad \xrightarrow{\text{time}} \end{array}$$

Time is here represented as a linear stream, and no limit can be placed upon the number of cause-effect pairs that precede the series shown or that follow it. In psychological theories such *limited* series as shown above are illustrated by linear sequences such as stimulus—receptor excitation—afferent neural excitation—central neural excitation—efferent neural excitation—muscle-fiber contraction. Something like this is implied in the Hullian system in the "linkage" between the stimulus process and the reaction. There is, however, no logical reason why the causal series should not be extended indefinitely at both ends. We shall presently consider this possibility in the example of the boy who is getting a drink of lemonade. This historical method of causality explanation is seldom carried through in explaining the phenomena of behavior, or indeed anywhere else. A certain part of the chain is delimited as "belonging" to a particular behavioral act or other phenomenon; and the remainder of the sequence is ignored. The fact that this can be done without being aware of any arbitrariness is itself an evidence that some principle other than linear causation must be at work. To find this principle is our present task.

But it is probable that events do not happen in the single-chain fashion just indicated. O may be a necessary condition of P , but it is usually not a *sufficient* condition. Other events, O' , O'' , O''' , and so on, must be present together with O in order to predict the occurrence of P , even though the absence of

any one of them might negate P . And similarly, each of these O 's may have behind it another compound set of earlier "causes." This situation, which is still linear in its sequences, is familiarly known as "multiple causation." It is represented in behavioral theories by such concepts as stimulus compounds, drive stimulus added to object stimulus, and attendant reinforcing conditions. Such a compounding of causes makes the definition of causation as a necessary and *sufficient* condition somewhat inapplicable right at the start.

But to proceed further, let us note that a number of manipulanda in the environment are frequently required for the description of a behavioral act. The chair, pitcher, and glass were prerequisites in the boy-lemonade example. Let us call the events of contact with such objects P , Q , R , etc. Now behind each of these objects there lies a cause-effect sequence that extends indefinitely into the past and ramifies in space. Let us take the boy's contact with the chair as P . An earlier O existed in the form of someone's placing the chair at the table. Behind this, at a still earlier time, was the matter of purchasing the chair, a happening which might have had multiple causation (O 's) in acts of conferring between the boy's father and mother and in the combined acts of sales clerk and cashier. Behind these was the act of the store's manager in having previously "stocked" the chair, and behind this were many happenings involved in transportation, each of which, in turn, had its multiple precursors. For example, prior to the "loading of a chair" there were acts of a number of workmen in a factory handling tools and materials in the making of the chair. And again, prior to *both* the materials and the tools lay multiple occurrences such as wood cutting, fabrication, transportation, and so on. As the linear series is traced back-

ward in time it is seen to spread out in space as a "regressus pyramid." By the time we have traced it backward only a few steps the number of O 's (and P 's) required to account for the contact of the boy's hand with the chair is so great that prediction from any one event has only a negligible value. Merely to illustrate the well-known regressus expansion (rather than for any intrinsic value it may have) Fig. 1

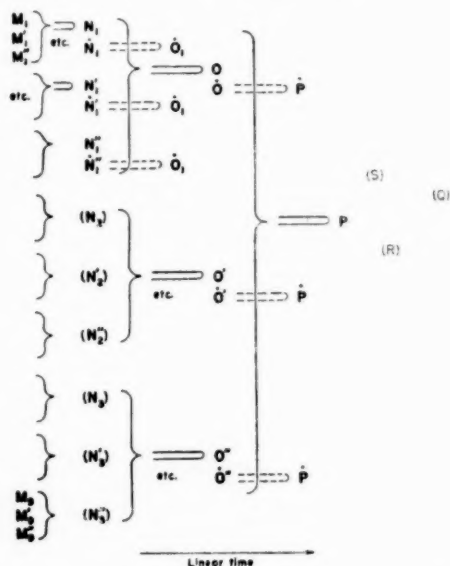


FIG. 1. Scheme showing the multiplicity of earlier events contributing to a final event P in linear causality. The letters in each column, with their braces, going backward in time from P , show three successive stages of the regressus. The symbol \supset indicates positive prediction (e.g., if O occurs then P will occur). A dot over a letter signifies non-occurrence of an event ("negative" prediction is involved). For illustrative purposes it is assumed that behind each event lies a "compound" of three other (necessary) events, no one of which, by itself, however, is sufficient for the prediction of the event in question. Thus no single event is both a necessary and a sufficient basis of prediction, or condition, of any other event. The number of contributing events at any given stage is equal to C^r , where C is the number of events in the compound (assuming that number to be constant) and r is the regressus stage taken.

is presented. The same analysis, of course, could be made with respect to the other objects, pitcher and glass, contacts with which were also a necessary part of the situation (*Q*, *R*, and *S* in Fig. 1). But now another difficulty arises. Each of these objects will have had its background pyramid series; and in order to produce the structure of behavior which we are considering the apices of these series must converge toward the interior of this particular dining room at a particular time when the boy also is there. How can we explain this convergence? We look in vain among the total antecedent events of the chair, the pitcher, the glass, and the boy, for any earmarks that will indicate a "destiny" of their coming together with the others at this time and place.

It might be argued that the boy's organism itself will supply the combining clue. The sequence is spelled out for us in the drive-and-stimulus-to-reaction chain of the mechanistic theories. But here again we shall meet with disappointment so long as we stick to the linear meaning of causality. First, we have the historical and environmental problem all over again in accounting for the *O*'s that lie in the boy's neural and muscular metabolism. But there is something more than that. There is built into the boy himself a kind of causal regressus pyramid. Neurologists have pointed out that as one proceeds (linearly) from receptor processes through central connections to effector units, a marked shrinkage in the number of elements or available pathways occurs. Many *O*'s are required for each ensuing *P* at every stage; and as one moves "backward" through these increasing sets of "multiple causes" the same sort of spreading effect is seen in the physiological regions as was noted in accounting for contacts with environmental objects. In terms of cybernetics

it is said that the loss of information merely in proceeding from neuron to muscle is 100 to 1 (3). And again, we have no way of describing by any causal series within the organism how the various elements or processes are integrated in the pattern of a single act. The structure simply "appears" among the ongoing elements as though it were not "caused" at all.

One further lesson can be gained from all this. We find that when we try to explain structure by temporal trains of causes and effects, we are usually faced by structure as an accomplished fact. The practically simultaneous excitations that existed in the boy's sensory cortex as he surveyed the possibilities of getting a drink of lemonade were all there together. The coordinations of separate movements by which he gained his end were also contemporaneous arrangements. Then too, as we looked back at the historical sequences outside the organism, the acts of human beings or machines by which the chair was transported, manufactured, and so on, these also were seen to be matters of spatiotemporal patterning. The behavior of one individual in any one of those aggregates was coordinated with and dependent upon the concurrent behavior of others. Patterns seem to flow not from linear trains of causes and effects, but somehow from patterns already existing. Structures come from structures; and in many cases the structures themselves, as wholes, seem to operate not sequentially but in a contemporaneous or concurrent fashion. Unless causality is already set in this "framework of structure" it becomes merely a pyramiding manifold of happenings without relevance to the structural problem. But when it is placed in such a setting is anything of importance added to the picture by the notion of causality?

III. TOWARD A GENERAL THEORY OF EVENT-STRUCTURE

It appears, therefore, that the attempt to explain structure through the customary time series of cause and effect is futile. Some other explanatory concept must be sought that will circumvent regressus and link up events in some kind of pattern. Explanations must lie in the approximate "here and now" rather than in the remote past. The only way to accomplish this seems to be to cut across the conventional and absolute "time stream." One can think of time as the duration occupied by the successive ongoing processes and events of a particular pattern *that closes itself through a cycle of operation*. Taking this idea as a clue, we shall begin the presentation of our proposed theory of structure by stating the following postulate: *All structures of events have a self-closing or cyclical character*. Instead of depicting the events of any aggregate, *M, N, O, P, or P, Q, R, S*, as a linear series, we shall always try to think of their occurrence as shown in Fig. 2. If *P* starts the series, the event succession returns to *P*, or at least to the region in which *P* occurs. If it returns to *P*, it may, thereafter, keep on

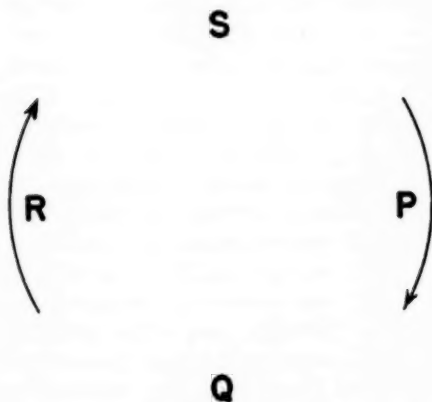


FIG. 2. Hypothesized arrangement of events in a (self-closing) structure. Arrows indicate event succession in "structural" time.

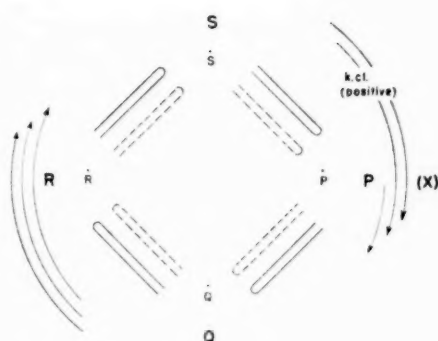


FIG. 3. Causality seen as dependent upon a prior hypothesis of structure. *P* is taken as the "starting" event. The cycle of events, as indicated by the arrows, repeats itself. *k.cl.* kinematic closure. It is maintained that only in such a predictively self-closing arrangement (structure) does "true causality" (i.e., positive and negative prediction by single event roles) occur. Contrast this figure with the compounding linear regressus of Fig. 1.

going in the same manner through repetitions of the cycle. If the cycle thus repeats itself, that would represent another "round" of "structural time." Time is thus always of the structure.

Though many such cycles of events may, of course, be connected by common events, regressus will be eliminated, first, because each structure preserves indefinitely its own characteristic pattern of ongoings, and secondly, because, though cycles can be linked indefinitely through space, their (repeating) operations may be actually "simultaneous" (that is, contemporaneous) in the linear meaning of time. The causality definition can here be reintroduced, if we wish, but this time in a structural rather than a linear setting. Figure 3 will illustrate this usage. The arrows suggest that this is a "repeating" cycle. We shall speak of the fact that the series returns to *P* (its starting point) as "kinematic closure" of the cycle (*k. cl.* in Fig. 3). In this case the closure is "positive" since it implies a continuation of the cycle. It

should be recognized, however, that the reintroduction of causality, as shown by the symbols, is merely a device for semantic convenience. It does not contribute anything beyond the postulate of structure upon which it is here already predicated. P is the sufficient and necessary condition of Q , Q of R , and so on, only because of the postulated self-closing character of the pattern.

But in order to fit the facts of behavior there must be introduced a second logical construction, having a type of closure prediction different from the first. It contains, at the last event position of the cycle, a new type of causality statement that is the inverse of the old; namely, if S occurs, P (the starting event) will *not recur*, and if S does not occur P will keep recurring (tangency of the structure at P to some "outside" structure, X , is presupposed in the latter case). But again we note that it is the structural hypothesis that is fundamental, not the symbols or concepts of causality. This construction is shown in Fig. 4, and kinematic closure

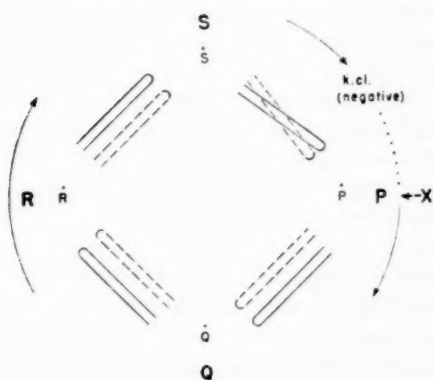


FIG. 4. Causality seen as dependent upon structure, with the predictive roles of S and \bar{S} reversed (as compared with Fig. 3). If S occurs, the starting event, P , does not recur; the cycle ends in the "region" of P . If S does *not* occur, P , assuming that it is "fed" by an outside event, X , keeps recurring even though the cycle as a whole does not close. The cycle is thus one of a nonrepeating type.

is here said to be negative. Such an arrangement typifies those cases in which the events of the cycle terminate upon a return to the initial region (non-repeating cycle). It may follow, in some cases, upon a series such as that represented in Fig. 3. An illustration of the latter condition would be found in food-taking behavior (by taking repeated mouthfuls) as the "hunger contractions," P (perhaps from blood-stream events), continue in the stomach. After a certain number of mouthfuls are taken, the situation represented in Fig. 4 would occur, brought about, perhaps, by a tangent cycle involving "nutrient" events in the blood stream. Another example of negative closure is to be found in the breaking of contact (event S) with a hot object, an event through which the initial event of stimulation (P) is prevented from continuing or recurring. If the reader wishes to generalize these schemes of positive and negative kinematic closure, he will find that they have a very broad application to behavior. It might, perhaps, be objected that if we do not come back to an actual reoccurrence of event P (Fig. 4), we do not have a true closed cycle. We do, however, have a closed cycle in a fundamental sense, since, in order for P *not* to recur, there must be a change or "deflection" of some sort *in the region of P* that negates P 's recurrence. For example, in the breaking of the contact of the hand with the hot object, the events do bring us back to a change in the state of affairs in the *initial region*.

We need one more addition to this purely logical stage of the model. Structures, wherever we take them in nature, probably never exist "in a vacuum." There is always "tangency" with other structures somewhere; and we can be sure that our structure

$\begin{matrix} S \\ R \rightarrow P \\ Q \end{matrix}$

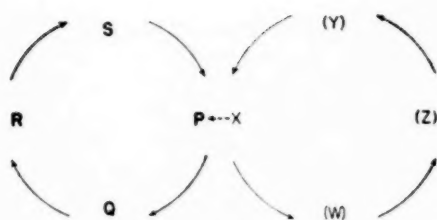


FIG. 5. A hypothetical event cycle (left) whose events (energies) are being augmented (or decreased) by interstructure with a "tangent" structure shown at the right.

is not only operating at its own "proper" level or frequency of events, but is capable of receiving events (i.e., energies) from adjacent structures, or, perhaps, of losing energy to them by some kind of pre-empting of its elements. Let us represent such tangencies, with application to both the earlier nonrepeating and repeating models, by the symbolization of Fig. 5. *X*, which is an event (event role) of another structure, is not here regarded either as a sufficient or a necessary condition of *P*, in the sense of *P* as an event role. The position *P* would be the site of *some* events without the aid of the tangent structure. *X* provides merely an "energetic reinforcement" of (or perhaps detracting from) the events occurring in structure *PQRS*. Linear causal regressus, in Fig. 2 through 5, has entirely disappeared. Aside from the structure *PQRS*, taken together with other structures (such as *WXYZ*) that may be immediately tangent to it, we have no interest in the nexus of events either in the past or the future. We do not care what the sources may be from which the contributing structure has, in turn, had its own energies supplemented, so long as the energies and the energetic contribution of that structure to the main structure can be determined.

So much for the general format of the logical model. It was intimated above that the letters of the model are really

event *roles*. The term "energies" was also used. These matters must now be explained. One of the defects of the notion of cause and effect, in addition to those mentioned, is that it implies that there is a specific event, *O*, invariably preceding and followed by another specific event, *P*. Empirical observation of event series, however, shows that such identifiable sequences of specific happenings do not invariably occur. We can say only that there is a certain *probability* that *P* will follow, and be preceded by, *O*. In fact, the occurrence of *O* (or of *P*) itself is a matter of probability dependent upon certain conditions. Instead of saying "if *O* then *P*," we might better say "probability of *O*, probability of *P*," and then add a third probability to express their *joint* occurrence or succession. Probability considerations, however, always imply that we must have a *fairly large number* of cases to observe. In order to determine probability or expectancy *many* events (or failures of events) and many successions or failures of successions must be counted under the classifications *O* and *P* and their interconnection. A further imperative reason for this pluralizing of the event concept within a role is the fact that we must make our model of structure general. It must fit all levels of nature (not just the macroscopic or molar level) or it will probably not fit any.

We might as well resign ourselves, then, to the necessity of treating events at the level of the microcosm (i.e., the most minute elements or happenings in nature) right at the start. And here, what has just been said of the indeterminacy of specific events and their succession applies with great force. There is, for example, no way of predicting that a certain minute particle, *a*, will collide with a certain particle, *b*, and hence there would be no prediction of a

one-to-one series or closed sequence of such specific events. The best we can say is that for any particle there is a certain probability, through time, that it will be in a specified region and hence that it will be "available" there for an event of encounter with another particle. Whether one of these ultramicroscopic events will take place between two *particular* particles is "upon the lap of the gods." There is, however, a certain "probable density," or *probable number* of such encounters, that can in the aggregate be predicted to take place in a given region through a given time. If this is so, there is also a degree of probability that such (probable) encounters will occur in *all* the regions around the (hypothetical) structure. Cause and effect, in the usual sense, must therefore be replaced by a statistical treatment of the matter. Where the number of minute events recurring in a certain region is very large, so that they can be observed "macroscopically" or, as it were *en masse*, we say that "the event *O*" (at our level of observation) occurs. And if one of these (macroscopic) "events" regularly follows another in successive spatiotemporal regions of observation, we say "If *O* then *P*"—or, "*O* is the cause of *P*." This, however, is only a crude statement and one not at all suited for the careful study and explanation of structure.

The formulation of events in terms of probability holds, of course, for all the event positions labeled *P*, *Q*, *R*, *S*, and *X* of the preceding diagrams. (In Fig. 4 the probability in position *P*, following the occurrence of events at *S*, falls abruptly toward zero.) In order to apply these concepts it is evident that we must always regard *P*, *Q*, *R*, etc. in our logical model not as single definite happenings, but as indicating *regions of space through time* in which events may or may not occur. They

are the "event regions" that are hypothesized as defining the structure. We must also be prepared to conceive the events in vast numbers in any region, and in ultramicroscopic as well as in macroscopic terms. But let us remember the further aspect of probability that must be incorporated in the design. Having dealt with the probabilities that events will occur in each of the regions *P*, *Q*, *R*, etc., singly, we now have to consider the probability that they will occur (with sufficient probable density) in *all* these regions of the self-closed structure taken together, that is, around the cycle, either simultaneously (if continuous) or in immediate temporal succession. As this probability approaches 1.0, it means that the structure in question is becoming increasingly clear and predictable. It is hypothesized that this is the phenomenon that takes place in both learning and perception.

Our *second* postulate, then, is as follows: *The observed occurrence of a structure of happenings is dependent upon (a) the probability of occurrence of events in each of the (event) regions of the structure, the regions being taken singly, and (b) the probability of the joint (or successive) occurrence of events in all the regions of the struc-*

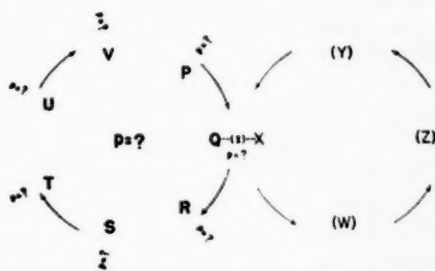


FIG. 6. A hypothetical event cycle (left) with indications that there are *probable numbers* of events (event densities) in the respective "event roles." Capital letters now signify space and time *event regions*, rather than single events. An interstructurant cycle is again shown at the right.

ture. This postulate is symbolized in Fig. 6 in which a larger number of regions is employed, the main structure under consideration being shown at the left. In this figure p indicates the probabilities within the single event regions represented by capital letters, and P the probability of the structure as a whole. As before, a contributing tangent structure is included for completeness. The I between Q and X represents an expected "interstructuranc ratio" between increases of the events in the regions of structure $WXYZ$ and those of the main structure. If the main structure is a food-taking behavior cycle, the contributing structure might be a cycle of events in the blood stream. Solid arrows indicate the temporal succession of events in the regions of the cycle.

Before going further with the model, which is still, for the most part, only in a logical stage, let us make some direct applications to the organism. Again we shall discuss the example of the boy getting a drink of lemonade, but shall ignore the contributory cycle (at the right in Fig. 6) and consider only the main structure. Figure 7 presents, for this purpose, a cycle having a greater number of event regions. Probability symbols are omitted *but should always be understood*, both for the event regions and for the structure as a whole. Let us first establish a clearer

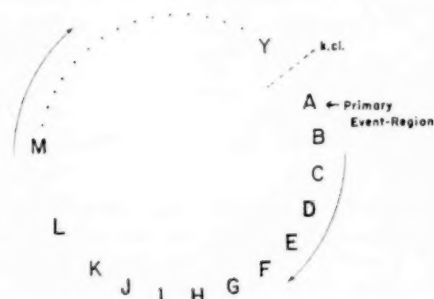


FIG. 7. A hypothetical event cycle of an act (see text)

definition of the term "event." This we shall define solely in terms of an indivisible, all-or-none, happening, as, for example, in an encounter or collision where minute particle elements come together (or, in relativity theory, to "near points") and then go apart again as they continue on their courses. Events may also be the sudden breaking of contact between elements. The dichotomous states involved in ionization and chemical interchange could come under this general definition of events. Such a definition may not seem at first to fit happenings like action across synapses; but it is believed that, if conceived at a fine enough, ultramicroscopic, level, it can be considered appropriate. *A single event, then, is a "dichotomizing," non-quantifiable, happening, and nothing more.* Its representation on a spatio-temporal model would be merely a point. We shall sometimes, for convenience, speak of "an event" in the singular when we mean a large number of such events (that is, an event role); but it should always be remembered that the letters of the diagram (Fig. 7) represent event *regions* of space and time in each of which a *large number* of these elementary events occur, giving us, when they occur at a probable density of threshold frequency, "the event" as *macroscopically* observed. Thus, as we know, a large number of stimulation points are involved on the retina or the skin as the boy sees the pitcher and the chair or as he touches these objects. A large number of afferent-neuron excitations and cortical synaptic events are involved in the sequelae of these stimulations. A large number of muscle-fiber-activation events occur at the efferent end plates. Many molecules of liquid strike the boy's throat as he drinks, replacing the many that have evaporated from the throat membrane. Each of these pluralities can, of course, be regarded as

the compounding of events in their more elementary status at the molecular or atomic level. It is useful to regard some one (or more) of the event regions of the model as a "primary" event region in that it represents the *initial* marked increase (or decrease) in events (energies) through tangencies with an outside structure. This procedure also locates the point of closure, which comes just before the primary event region (*k. cl.* in Fig. 7).

Let us now represent the event of "throat drying" or, more exactly, the region in which many microscopic events of tissue change occur as the body moisture evaporates, by *A* (Fig. 7). *A*, then, is the primary event region. Omitting vision and some other aspects for simplification, we can now assign to the other letters, as regions, approximately the following event roles: *B*, stimulation of receptor(s) in throat membrane (from the drying); *C*, excitation of afferent neuron(s); *D*, excitation of neuron(s) at synapses in the central nervous system; *E*, excitation of other neuron(s) at other synapses; *F*, excitation of efferent neuron(s); *G*, excitation of extensor arm muscle fibers at end plates (hand here moves forward toward the chair); *H*, contact of hand with chair; *I*, stimulation of proprioceptors and tactual receptors by this contact; *J*, *K*, *L*, *M*, etc., afferent, central, efferent, and muscle-fiber excitations (as hand *closes* on the back of the chair).

From this point on let us simplify matters by conceiving further elements of the series (dotted line) as representing other neural, synaptic, muscular, receptor, and bodily contact events as the chair is placed in position and as the boy climbs up, takes the pitcher, pours a glass of lemonade, and tips the glass up at his (open) mouth. Eventually we come to an event (let us call it *Y*) at which the liquid encounters

the throat membrane and the "moistening" of the throat represents a partial negation (diminution of energies) in the "drying" events of the tissue. With kinematic closure at *Y*, then, we come back to the starting (or primary-event) region. Since the tissue-drying events are only partially reduced in number, a repetition of a part of the cycle will occur (positive *k. cl.*). This part, not distinguished in the diagram, could be charted as a "component" cycle involved in the taking of *successive swallows*. Finally, as the "swallow cycles" continue, event densities at *A* are reduced to a state at which the whole cycle is energetically "in equilibrium." Events at *A* now cease to occur (negative kinematic closure) and with their nonoccurrence the remainder of the cycle is negated, at least in so far as the superthreshold level of "conduction" and overt action is concerned. It should be noted (though it cannot be explained at this point) that some of the event regions in the cycle may be more "readied" than others. That is, they may already have an event density approaching threshold, though the remainder are not yet at a probability stage in which the structure as a whole can appear. *Continuation* of the occurrence of events in the "readied" regions (see later) would guarantee their presence when needed for the total structure. (Many considerations that would need to be included in a full structural diagram, such, for example, as a coordinated cycle of mouth opening and [later] closing, have been omitted to simplify the illustration. Tangent "feedback" cycles of optical and opposed neuromuscular ongoings have also been omitted.)

There is still one feature that must be added to the logical model before it can be given its full physical or organismic significance. The events *A*, *B*, *C*, etc. of Fig. 7 are really *connected*.

Is such connection only a matter of abstract joint probabilities, or is it "physical"? Theory and common sense both require the latter answer. If events represent encounters between minute (hypothetical) elements that collide or come to "near points" in their ongoing, then the only way in which such events can be connected is by the fact that one ongoing element, after it encounters its opposite, continues and makes an encounter with another ongoing element. To illustrate such ongoing in our example let us recall that water molecules *travel* in space as they "evaporate" from the throat membrane. Some kinetic feature is probably also present in the molecular activity of receptors, connecting stimulus event with afferent-neuron-excitation event. A neural impulse represents a *whole train* of *minute cyclical ongoing*s (of ions) through and along the neural membrane, connecting the events of excitation at one end of the neuron with events of excitation at the other. The more grossly perceived ongoing of the hand as it raises the glass, represented at a microcosmic order by neural and muscular ongoing cycles, connects the event region of "glass grasping" with that of "glass tipping." The flowing (or fall) of the liquid is the descending portion of a gravitational cycle of ongoing and connects the events of displacement of the liquid as the glass is tipped with the events of the droplets striking the throat. In this way a connection of events is provided by *each ongoing role*. There is also a connection of the ongoing *by* events. There is, in other words, a structure of both ongoing and events. A kinetic or "motion" aspect must therefore be added to the elements of the model. To this task we shall presently return. Again we note, from the format of the model, that these several ongoing (which are in general themselves cyclical) and the events by

which they are connected and which they connect form, when taken together, an *over-all* cycle (Fig. 7). And again, the *numbers* in which events occur in each of the regions separately, *and therefore the probability of the behavior structure as a whole*, are a matter of the (sub)microscopic probability density at the event regions.

It will be seen that regressus is completely eliminated from the model. All that is needed is that there be a sufficient space and time availability of ongoing elements at event regions necessary to constitute a structure. The pitcher of lemonade might have been on the buffet many hours, or it might have been there for only a thousandth of a second before the boy's eyes turned toward it or his hand encountered it; and it could have come there through any one of an indefinite number of pyramiding lines of "causality." These considerations are without significance for our present problem. The only thing that concerns us about the concentration of molecular cycles we call the pitcher and its contents is the *probability* that such a "concentration" will be present at a time and place that will permit encounters to be made with it by the "ongoing elements" of the boy's hand. Since, however, all events have a *certain degree* of randomness, this requirement makes room for "approximations." If, for example, the pitcher had been in another room, or had been available only just *before* the maximal drying (events) of the boy's throat occurred, the probability that a lemonade-getting-and-drinking structure would have occurred would have been *less* (though still not necessarily zero). And the same can be said for probable density at all the regions of the structure, including its tangencies with other structures, such as those of blood-stream events, which lie inside the organism.

It is unnecessary to ask what "makes"

or "brings about" all these regional event probabilities and their concurrence in time and space. They are implicit in the empirical situation itself. If they were not, the structure (that is, the behavior) would not occur. No "field expectancy," "organizer," or "entelechy" is needed in a theory of event structure. We can think of the pitcher of lemonade, the glass, the chair, the internal systemic changes that occur in "thirst," and all other relevant situational features as "*bounding conditions*" of the lemonade-taking-and-drinking structure. These bounding conditions, which are themselves self-closing *structures*, increase, by imposing space-time limits upon the freedom of adjacent ongoings, the probability that the (bounded) ongoings of the cycle under consideration will come to events at intervening regions (including synaptic areas) with a density above the macroscopic threshold; and in so doing they help to bring about the self-contained and self-closing structure of the (bounded) behavioral act. The implications of this conception for a new and more comprehensive theory of learning are evident. The notion of the probability density of a structure's occurrence under given bounding conditions might be substituted for such earlier notions as sign gestalten, "stimuli" linearly evoking "responses," strength of associative S-R linkages, and the selection and fixation of neural pathways. The difference between continuity and noncontinuity learning may be merely a difference in the shape of the curves of distribution of the increasing *probable structural densities* of the act as the experimental situation is repeated, the curve in each case being plotted along a continuum of structurizations or "trials" with experimentally imposed bounding conditions that are different in the two cases.

Let us now return to the problem of

representing the motion of ongoing elements in the model. In order to supply this feature, the ongoings must be represented as *curves*, each suggesting *continuous* motion; for it seems probable that at the minute levels of nature particles *are* in continual motion and that their motion is cyclical or vibratory. Moreover, nothing ever starts from a position of ascertainable "absolute rest" or proceeds to another point of absolute rest. In fact, if we are to get away from a purely static conception, points in space and time are *definable only* as the points of conjunction between ongoings or motions. Let us try to diagram the situation, at first, without any consideration of where the ongoings start or of their ultimate destiny. Let us also avoid trying, for the moment, to link the model too closely to neurophysiological considerations. Figure 8 shows six ongoings (broken lines *i* to *v*, and *r*) with event regions between adjacent ongoings. Evidently we must assume that there is something in each case that "goes on." Without trying to be more specific, let us postulate, for the purpose of the geometry of the model, that it is, *in the last analysis*, a "continuance-head" (or particle?) of the smallest conceivable magnitude. (If the diagram were adapted to our present example, the *compounded*, or higher order, ongoing elements would represent such features as ion cycles in neural impulses, muscle-fiber molecular lengthenings and shortenings, flowing of the liquid, and so on.) The "event-connecting" segments of these ongoings occupy the central portion of Fig. 8. These ongoings must, of course, be conceived in *plural number* for each ongoing role; and we know, in fact, that this is actually the case. In any act of behavior, after many points are stimulated on a receptor surface, many neural impulses travel, as it were, "in parallel," many cortical fibers are involved, many

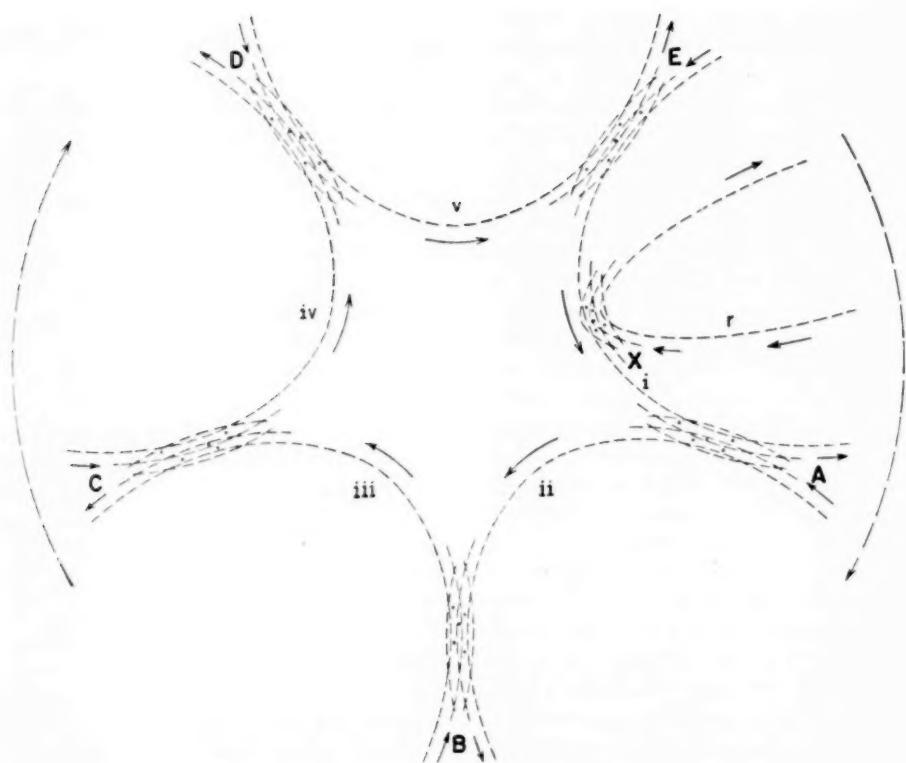


FIG. 8. The structure (?) of a behavioral act, with its ongoing connections between the event regions, as it would appear if the ongoing connections were conceived as indefinitely extended (i.e., linear). *i-v*. Ongoing connections (really pluralities or "sheafs" of ongoing connections, as shown at event regions). *A-E*. Event regions of the (assumed) structure. Dots in the regions represent events. *X* is an event region where an interstructural ongoing, *r*, contributes events (energies) to the main structure. If the arrows in the central part of the figure really identified a temporal succession, the figure would symbolize a (self-closing) structuring of events; but it would not represent a completed structure of events and self-closing ongoing connections. Structure, in other words, is *not pervasive* in this model.

muscle fibers contract in their common role in a single effector movement, and so on. Note that this feature is suggested in Fig. 8 by the duplication of the lines for the ongoing connections (shown only in the areas of the event regions). Events between these multiplicities of ongoing connections, whose probable density or numbers in the several regions underlie the structural probability of the act, are indicated by dots. Ignoring ongoing *r* and region *X* for the present,

we shall regard ongoing *i* and region *A* as our starting point. The short solid arrows indicate the direction or sense of the ongoing connections; and they show a temporal clockwise succession of event occurrences in regions *A* to *E* and back to *A*, as indicated by the longer broken arrows at the margin of the figure. We now have a cyclical structure of *events* (but of events only), connected by continuous ongoing connections which themselves are *not* structured but extend (small arrows

at the periphery) from an indefinite past into an indefinite future. Let us see if this construction is satisfactory.

At least three provisions must be made for any structural model of behavior: (a) It must accommodate itself both to relatively stationary and to "successive" patternings. In some structures, as, for example, in perceptions of objects, all parts of the object seem to be perceived at once. On the other hand, many typical structures of behavior have a cyclical *succession* of happenings (as we have shown, for example, in the case of the boy and the lemonade). (b) In order to meet the latter requirement there must be some suitable arrangement for the timing of the ongoing elements so that the events (regions) will occur in proper order. (c) Many behaviors not only occur in sequential arrangements but are sustained, in the sense of a repetition (of their complete cycles), through time. This feature, which is called *steady state*, requires a continual input contribution of events (energies) from some explicable source.

An examination of Fig. 8 shows that the construction there presented does not meet these requirements. Time coincidence of ongoing elements in an event region and time *sequence* of the *successive* regions are shown but not explained; and in order to account for them we would probably have to invoke some special "organizing agency." "Static" or simultaneous event structuring could be accommodated if we consider that, instead of separate "volleys" of particles coming to events in the region, we have a *continuous flow* of particles along the course of each ongoing. Events would then be occurring in the five regions (*A . . . E*) in practical simultaneity. The situation so represented might be equivalent to an "equilibrium" of the structure. If we should wish to turn the picture into a

kind of succession, we could do so by adding an "input source" in the form of another ongoing stream (*r* in Fig. 8) which comes to an event region (*X*) with one of the ongoing streams of the main structure. If the *increases* in numbers of events introduced into the cycle at *X* are passed on from one stream of ongoings to another, we would then have *successive* increases of density in regions *A* through *E*, and thereafter, under conditions of positive kinematic closure, around the cycle repeatedly. We can suppose that there will be some sort of output tangency to keep the state in balance, so that the *full* energies continually being added to *A* from *X* are not passed back from *E*, via *i*, to *A*, but only a portion of those energies. This interesting explanation, which may be called a theorem of "conduction," gives a basis for steady state. So far so good. But a difficulty arises in these explanations both of equilibrium and of steady state. Some continuous source for the ongoings (i.e., ongoing elements) is needed in both instances. Where, for example, does ongoing *r* get its supply? Either we must think of these sources as the ongoing lines themselves, extending from an infinite past, a conclusion at odds with the temporal self-containedness of phenomena, or else we must suppose that they are derived, for each of the unclosed ongoings in the figure, from tangencies with other, more remote, sets of (unclosed) ongoings. In the latter case we begin to slide back into the old multiple regressus.

The difficulties here encountered can be summarized by saying that we have been trying to build a self-closed structure out of materials that are themselves unstructured. One cannot make a true structure out of open-ended lines that merely "butt against" one another as in Fig. 8. Structure must be pervasive if it is to exist at all. It must be composed of units (in this case, on-

goings) that are themselves self-closed. We need, therefore, to suppose that the six ongoing roles of Fig. 8, instead of extending out indefinitely in space through time, have a curvature throughout their course and return upon themselves. Could we say, perhaps, that the ongoings follow the curvature of the continuum of space-time?

Without trying to elaborate the last proposal we shall pass at once to a new and final design in accordance with the idea just expressed. It represents merely an extension of our first postulate (self-closedness) down to the lowest orders of the microcosm. For cartographic convenience only six subcycles of ongoings and six event regions will be used. Actually, of course, there would be a *very large number* since the model must be conceived, ultimately, in microcosmic terms. In Fig. 9 we have shown this structure, in principle, as 1, together with out-structural tangencies with two other structures, 2 and 2a, affording an input or added increment of event density, and an output, respectively. The legend will recall the meanings of the various symbols, and the earlier organismic details given for the boy-lemonade episode in connection with Fig. 7 (or any other behavior by which the reader might wish to test the model) will supply illustrative content. In applying the construction of Fig. 9 it should be remembered that there is no limit upon the number of subcycles of which the structure under consideration can be composed. We now have a consistent theoretical model of a structuring of events, in this case the structuring of a behavioral act. It consists (see structure 1) of a set of subcycles of ongoings and a cycle of events (event regions) between, and provided by, the ongoings—a cycle of cyclical ongoings and events. Though the evidence cannot be here fully presented, but must rest with the specifications of the micro-

cosm previously mentioned, the writer believes that the gross and finer facts of neurology, physiology, and environmental contacts will justify the use of such a model for the description of behavior. It is also consistent with structural principles to assume the existence of "*higher*" orders of structure, that is, of structures that are composed of cycles of cycles of cycles, and so on. For example, conceive a larger cycle made by joining a number of cycles such as 1 in Fig. 9. Structure can thus be pervasive and can provide an explanation of the various levels or "hierarchies" of nature. (As the reader will see, this was impossible with constructions like that of Fig. 8.) Such higher orders, for example, might describe the *collective* or "*social*" structurings of the behavior structures of individuals; and such a description could well replace the present ambiguous and unsatisfactory term "group."

The three requirements listed earlier for a structural model are all met by Fig. 9. Equilibrium or a "static" structural condition is achieved under certain conditions by the fact that the ongoings of each of the subcycles (see smallest arrows) return again after one event region to the region where they had been just previously. With a continuous flow of elements around each of the ongoing subcycles, a virtual simultaneity of events would occur throughout the structure.² But whenever a sudden increase in the probable density of events is given through a tangent input structure (2 in Fig. 9), the equilibrium of the structure is disturbed; and this increase, beginning with the primary event region, that is, the re-

² The vibrations of molecules in solid, colloidal, or liquid states might be an example of such an equilibrium. Postural-tonus is also suggested, though this may also have something of the character of a steady (repetitive) state of the whole cycle.

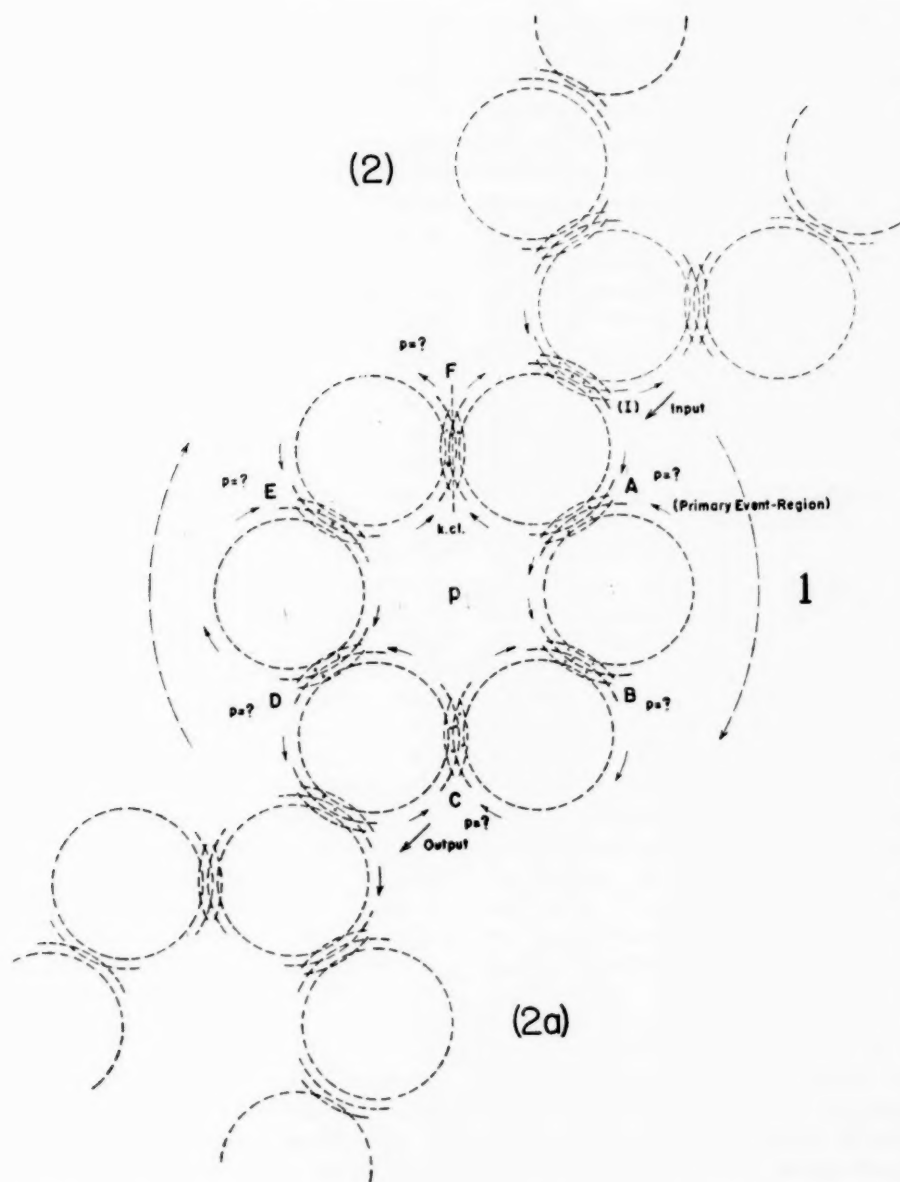


FIG. 9. Hypothetical diagram of the event structure of a behavioral act. 1. The event cycle of the act under consideration. 2, 2a. Tangent cycles providing "input" and "output," respectively. Broken line circles (see 1) are the self-closed ongoing of subcycles, with duplications to show plurality in each "role." Small arrows show direction of these ongoing. A-F. event regions of the act cycle. Dots indicate events (as of encounter) between the ongoing. p , p , etc. probable density (or number) of events (energies) in a region. p probability of the occurrence of act structure as a whole ("structural probability"). $k.c.l.$ kinematic closure (+ or -). Broken line arrows show the order of succession of energetic increments received from structure 2 as they are passed along through the event regions. I represents "interstructuranc" of structures 1 and 2; it also refers to an "interstructuranc index." (Ongoing and event details are completed for only a portion of the figure.)

gion first increased, is displaced around the cycle of event regions (broken arrows). Such a situation occurs in the full phasic sequences of a behavioral act and perhaps even at subthreshold density levels. With *repeated* rounds of the event cycle, should these occur, we would have a steady state. The problem of coincidence timing is solved by the reverberating character of the subcycles, which, with particle-elements continually ongoing, provide a *continual* availability for events. Succession in time could be guaranteed, in the sense of a succession of energetic increases, by the passing along of increments from one event region to the next in the steady-state condition mentioned above. There is now no difficulty in providing for the *source* of the energetic units of the cycle, or of the energetic contributions made to the structure by adjacent structures. For we are not faced by a regressus to indefinite origins. The subcycles themselves, and the cycles of the tangent structures, continually possess availability for events in the immediate present by the self-closing and repetitive character of their ongoing. And we need to consider only the event cycle of the behavior in which we are interested plus the cycles that are immediately adjacent to it.

The task outlined at the beginning of this article has now been, at least in part, accomplished. It has been shown that by laying aside linear models and linear causality, and by passing over into a logic of structure supplemented by probabilities, we can arrive at a fairly clear conception of "another type" of natural law. The conception of such a law is essentially "geometric" rather than quantitative. Our statement of this (hypothesized) law, however, has been as objective in principle and precise in reference as statements of laws

that are based on "abstracted" measured quantities. The writer maintains that the proposed paradigm is general for all acts of behavior and that it even suggests a unifying bridge across the "hierarchy of the sciences." Event-structure theory, though it includes quantitative considerations, nevertheless rests upon a foundation that is basically nonquantitative. Consider, for example, such concepts as the self-closedness of ongoing, the forms of kinematic closure, the indivisibility of an event, and the compounding of structural orders. No statements of dimensions, counts, or measurements can convey the full and essential meaning of these conceptions. The system is essentially a geometry of ongoing and their interrelations at "event points." In short, it is a theory of *structural kinematics*. But at the same time a *place* for quantities and covariational laws is provided. For quantities are probably related, in the last analysis, to energies; and energies can be represented, as we have shown, as *structured potentialities for numbers of events*. Hence the entire conception embraces also a theory of "*structural energetics*" or "*structural dynamics*." It is important to note, however, that a rational place for quantitative laws could not have been provided without the (nonquantitative) structural kinematics.

The writer believes that the event-structural paradigm will be found to be applicable to all organismic phenomena at the biological and physiological, as well as at the behavioral, level, and that it will apply also to collective or social aggregates. Obviously the theory, as here proposed, is only in a pioneering stage. The account here given is also merely a general outline from which many details have had to be omitted. Many questions arise for which answers must later be provided; and a large amount of work, both

experimental and theoretical, will be needed in order to arrive at a true appraisal of its validity. A further, though not a final, step in the theory's presentation, in which some of these questions are answered and additional properties and principles of structure appear, will be undertaken in a forthcoming volume (1). The theory will there be approached and exemplified through the facts and theories of perception.

IV. QUANTITATIVE ASPECTS OF EVENT-STRUCTURE THEORY (STRUCTURAL ENERGICS)

It is hoped that nothing that has been said will be construed as a failure to realize the importance of quantitative methods in the work of science. The quantitative and nonquantitative aspects of investigation should proceed together. An attempt has therefore been made to deduce some quantitative hypotheses from the over-all model, and, through the aid of students and associates, these hypotheses have been tested experimentally. Space will permit only the briefest description of this work.

The major hypothesis, thus far, has been concerned with predicting the amount of energy in a structure (cf., for illustrative discussion, structure 1 of Fig. 9). Such energy or probable event density is conceived under two aspects, the "autonomous" or "proper" energy of the structure, which is called the energy of "*structurance*," and the energy that is contributed to it by other structures in "*constructurance*" with it, such, for example, as that shown at 2 in Fig. 9. There may be few of these or a great many, but all must be considered. Such contributions constitute the energy of "*interstructurance*" or the "interstructurance increments" provided to the main structure by its surrounding tangent structures,

or "*manifold*." But the structures of the manifold may, in some cases, be "*antistructurant*," rather than constructurant, to the main structure. That is, there may be some kind of kinematic "deflection" which *deprives* the main structure of energies by decreasing its probable density, so that, instead of having the two structures increase in energies together when the tangent structure receives increases from its manifold, we may have a *decrease* in the energies of the main structure as the tangent structure is increasing. The contribution to the main structure from manifold structures (e.g., from structure 2 in Fig. 9) will, in such cases, have a minus sign. The constructurant relationship represents instances of *facilitation* in the biological, behavioral, and social realms, while the antistructurant relation represents *alternation*, *inhibition*, or "*conflict*." Furthermore, it is conceived that such additions or subtractions of energy proceed by constant (kinematically determined) ratios to increases in the manifold structure. This ratio (increase in the main structure divided by the attendant increase or loss in the manifold structure) is called the "*index of interstructurance*" of the manifold structure to the main structure. The interstructurance index (suggested as applying at 1 in Fig. 9) is specific to the pair of structures concerned, is limited to unity, and may be either positive or negative. (Actually it should represent a function expressed by a curve showing the relationship of the two variables.) The "output" quantity of the structure (for example, to 2a in Fig. 9) can be neglected in this problem, since our objective is to find the amount of energy which is *available* in the main structure at a given (present) time, either for self-maintenance of the structure or for being passed on to adjacent structures. Hence the *total amount* of energies of a structure, so

defined, at any given time, is a function of (a) its proper or "structurance" energies, which represent a sort of mean of its operation through time, or, if one prefers, a "homeostatic level," and (b) the sum of the interstructurance increments (or decrements) that are being received from its manifold. It is believed that these increments or decrements are given by the structurance energies of the manifold structures concerned, the latter, however, being first weighted, respectively, by their indices of interstructurance with the main structure. In the symbolism of the diagram (Fig. 9) this total amount of energies corresponds to the total number of dots in all the event regions of structure 1 (remembering that these have been augmented, or detracted from, through interstructurance with manifold structure 2 at I).

The generalized equation that has been derived in this manner from the postulates, kinematic concepts, and definitions of the theory is stated as follows:

$$E_1 = f(S_1 + S_2 I_{2 \rightarrow 1} + S_3 I_{3 \rightarrow 1} \cdots + S_n I_{n \rightarrow 1}),$$

where the subscripts indicate different structures (1 being the main structure under consideration), E_1 is the total energy of the main structure, S is the "proper" energy or structurance value of a structure, and I is the index of interstructurance of a (manifold) structure to the main structure as shown by the subscripts and arrows. Structures 2 to n represent all the structures of the manifold; and the summation is, of course, algebraic. This equation is known as the structural-energies (or structural-dynamics) formula. In using the formula, manifold structures considered on logical grounds to be very low in S or in I are usually omitted as having no appreciable value in the summation. Should the value of the equation turn out to be negative, E_1

would not represent "negative" energies occurring in structure 1, but presumably (positive) energies that were being expended in a structure or structures *antistructurant* to 1. The manifold of structures (shown to the right of S_1 in the equation) can be broken down into various classes or types, and the contributions of these types, separately, to the dependent variable, E_1 , can be determined. For example, in predicting the intensity with which an attitude is held by an individual (energies of an attitude structure), as E_1 , three types of manifold structures have been used, viz., personality-trend structures of the subject, small "face-to-face" collective structures into which his own behavior is structured, and his larger organized, or institutional, structures. The total summation, as provided by the equation, is hypothesized as giving a fairly accurate prediction of strength for the attitude concerned.³

The testing of adapted forms of the equation has thus far been carried on by simple correlation procedures involving E_1 and the manifold summation only. Methods of measurement for S and I , and sometimes for E , have been limited to subjective scaling, but with carefully prepared forms. The actual "energies" implied in E and S have thus far had to be inferred from the reactions on response forms or in an experimental situation. The equation has been tested (or tested in part) in nine independent investigations, largely at the structural orders of personality and social psychology, and representing cases from the fields of propaganda, attitudes, personality characteristics, job adjustments in industry, insight, learn-

³ Since the attitude represents a "meaning structure" within the individual, it is assumed that the two types of collective structures interstructurant with the attitude cycle are also represented at the intraorganismic level (i.e., as meaning cycles).

ing, and custom behaviors of American males. In all but one of these investigations (an early one in which it now seems that the hypothesis was not adequately stated) significant correlations, ranging approximately from .20 to .80, were obtained. Worthy of note here are the variety of structures whose total energies were to be determined, the considerable number of independent raters often employed for determining the different variables, the very large number of manifold structures whose increments entered into the summation for each subject in most of the experiments, and the fact that these interstructure increments were either positive or negative and hence involved subtraction as well as addition of energies in computing the predicted E . The consistent experimental support given the hypothesis in the face of these complex conditions seems surprising and would tend to suggest that, although it was developed in a highly general frame of reference, or perhaps *because* it was so developed, the theory does accommodate itself to the quantitative facts of human behavior.⁴

⁴ It is hoped that published reports of these studies, presenting the detailed findings and showing the methods of collecting structural information and rendering E , S , and I operational, will soon be forthcoming. The writer also hopes that further investigations of the hypothesis will be undertaken by others.

V. SUMMARY

The adequacy of quantitative laws and the common practice of thinking in terms of linear cause and effect, as methods of dealing with the universal problem of structure, are questioned. The need of approaching the study of structure in its own right, and by independent and (at the start) nonquantitative concepts, is stressed; and a general conceptual model of the structuring of ongoing and events is presented and illustrated in the field of behavior. The combined headings of structural kinematics or geometry (nonquantitative) and structural energetics (quantitative) are here found useful. An equation for the latter is developed, and experimental findings thus far obtained in its testing in the field of psychological phenomena are briefly discussed.

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THE VISUAL PERCEPTION OF OBJECTIVE MOTION AND SUBJECTIVE MOVEMENT¹

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The perception of motion in the visual field, when recognized as a psychological problem instead of something self-evident, is often taken to present the same kind of problem as the perception of color or of form. Movement is thought to be simply one of the characteristics of an object, and the only question is "how do we see it?" Actually, the problem cuts across many of the unanswered questions of psychology, including those concerned with behavior. It involves at least three separable, but closely related problems: How do we see the motion of an object? How do we see the stability of the environment? How do we perceive ourselves as moving in a stable environment?

MOTION, STABILITY, AND MOVEMENT

The first problem concerns the visual perception of a moving object. It seems fairly simple as long as one considers a motionless eye. The stimulus condition for a moving object is the moving sheaf of light rays reflected from it. The retinal image accordingly moves relative to the retina and relative to the background image of the environment. The stimulus for visual movement is retinal

movement. This definition is adequate, however, only for a fixated eye. It fails when we consider that the eye normally follows a moving object with a rotary pursuit movement that keeps the image of the object fairly precisely on the fovea. The background image then moves across the retina, but the object image does not. In this case the stimulus for the impression of motion is not so easy to define. A response is going on, and stimulation mediated by this response may enter into the picture. One might assume that movement of the object image *relative to* the background image but not the retina was the effective stimulus. Perhaps the observer senses the motion of the background and perceives the relative motion of the object. Or one might just as well assume that movement of the eye itself relative to the head or relative to the background image but not the object image was the effective stimulus. Perhaps the observer senses the movement of the eye and thereby perceives the motion of the object. The alternatives are highly debatable, but for either one a difficult theoretical question arises: Why do we perceive a motion of the *object* in the environment instead of a motion of the *environment*? This leads to the second problem.

The second problem concerns the visual perception of a stable environment. Why does the world appear motionless, and what are the stimulus conditions for this perception? It is just as much a problem, if less obvious, as the first. Superficially considered, it appears simple for the case of the fixated eye: a motionless image yields a mo-

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tionless percept. It becomes difficult, however, for the case of the moving eye. Why does the phenomenal world not move during an eye movement? The eyes perform saccadic or exploratory movements without ceasing during waking life; they perform compensatory movements whenever the head moves; and they perform pursuit movements whenever a moving object catches the attention of the observer. Since the image of the environment moves across the retina during all these responses, the world should seem to move. It may be noted that with certain unusual types of eye movement an observer will report that the world *does* seem to move; examples are the after-nystagmus caused by bodily rotation (or other causes) and the artificial movement of the eye caused by pushing it with one's finger (11). During normal eye movements, however, the world does *not* seem to move, and this poses a question.

There are still other reasons for rejecting the simple hypothesis that a motionless image yields a motionless percept. They appear when we consider what happens when the *observer* moves.

The third problem concerns the visual perception of locomotion in a stable environment. We perceive not only the motions of objects but the movements of ourselves; the performance of fielding a baseball illustrates both. In the case of active locomotion, such as running, there is, of course, a large component of kinesthetic stimulation from the proprioceptors that accompanies the purely visual stimulation from the retinas. But in the case of passive or involuntary locomotion, such as riding in trains, automobiles, and planes, the kinesthetic component may almost wholly drop out. The visual component of stimulation results from the fact of motion parallax, and consists of differential motions of different parts of the image.

The writer and collaborators have recently given a mathematical description of this kind of stimulation for the general case of what is called *motion perspective* (8). The fact that it has to do with the perception of space has long been recognized, but the fact that it also has to do with the perception of *locomotion* is less well understood and deserves emphasis. The visual field during forward locomotion seems to expand radially from a point of focus on the line of locomotion. The optical geometry of this expansion is perfectly definite. The retinal image undergoes a deformation that can be neatly specified in terms of differential angular velocities. This retinal motion reaches high magnitudes during rapid travel, and there is reason to believe that it is the important factor in the performance of landing an aircraft. The apparent expansion of the visual field has been noticed by nearly everybody in driving an automobile. The question that arises is why the visual *world* does not seem to expand but instead seems to appear rigid, with the observer moving instead. The flier is never confused by the impression that his runway is behaving like stretched rubber.

It is worth noting that there are special cases of visual stimulation in which it *does* become equivocal whether the visual scene is moving or whether the observer himself is moving. If one sits looking through the window of a stationary railway train at another train on the adjacent track, and if one of the trains begins to move slowly, the impression of moving self with stationary scene may give way to that of stationary self with moving scene, or vice versa.

The three problems of the moving object, the stationary environment, and the moving observer are evidently interrelated. Objective motion is con-

nected with subjective movement,² since both stimulate the retina. The motion of an object, the movement of the eye, and the movement of the observer himself may alter the retinal image in different ways, but they all alter it. They are all inseparable from the problem of how or why we see the environment as stationary both when its image is altered and when it is stationary on the retina. One thing is clear at least: the kinetic experience in general involves the problems of so-called space perception.

EXPERIMENTAL EVIDENCE ON THE PERCEPTION OF MOTION AND MOVEMENT

A survey of the established facts about the three problems may clarify them and even point to solutions. The experiments are not numerous, some of them are unfamiliar, and they have seldom been considered together.

Motion of an Object

Apparatus employed. Experimental studies of visual perception necessarily depend on devices for systematically

presenting light to the eyes of the observer, that is, methods of systematically varying his retinal images. In the case of motion, not many such devices have been successfully built. The types of apparatus for inducing controlled impressions of objective motion are approximately as follows: (a) the stroboscope and the variants of this device, used to study apparent motion; (b) the moving belt viewed through a window or aperture, used to study so-called "real" motion, or to induce the waterfall illusion; (c) the rotating disk with a spiral, used to induce the impression of an expanding or contracting object and the negative afterimage of this impression; (d) the device of casting the shadow of a physically moving or rotating object on a translucent screen, the deforming shadow inducing the impression of a three-dimensional object in motion; (e) the device of rotating a disk with spiral lines behind a slotted screen, inducing the impression of objects moving along the slot. Practically all that is established about the perception of motion comes from one or another of these experimental methods. However, a novel device for presenting multiple complex motions on a translucent screen has recently been described by Johansson (12). One might suppose that the animated motion picture would have been used for controlled experimentation by psychologists, but it has scarcely been tried (5, ch. 2). There have also been a number of setups with luminous spots in a darkroom, one or more of which are put into relative motion. This latter experiment, like the autokinetic illusion, is relevant to the problem of the stability of the environment as much as it is to the motion of an object.

Stroboscopic motion. The only large body of evidence based on these devices comes from the stroboscope. It is said to yield "apparent" motion as distin-

² In this paper, for lack of a better terminology, the word *motion* will always be used to refer to change in position of an object, and the word *movement* will always refer to change in position of the observer's body in whole or part, that is, a response. Both may be visually perceived. The responses with which we are concerned are chiefly eye movements and locomotor movements. Movements of the limbs and hands are also important since they constitute a large part of behavior (gestures, manipulation, tool-using), and most of these are also visually perceived. In them, however, the kinesthetic component, the muscle sense, is obviously important, and the visual component cannot be isolated for analysis as it can for locomotion. They are practically never passive or involuntary, as locomotion can be. They will not be considered here. Nevertheless the writer believes that the visual feedback is just as important for motor performance as the bodily feedback, and that "visual kinesthesia" should be recognized along with classical kinesthesia.

guished from "real" motion, and the stroboscopic effect is often loosely referred to as the phi phenomenon. The stroboscope has evoked much research, probably because it demonstrates that a physically moving object is not necessary for an experience of motion, and because this seeming paradox has prompted psychologists to formulate controversial theories in order to explain it.

The important fact about stroboscopic motion, for present purposes, is that the stimulus is intermittent but that when certain relations hold, the perception of motion is the same as if the stimulus were *not* intermittent. As Troland asserted, "a perfect motion impression can be aroused without any actual motion of an object by the discontinuous substitution of one object for another at progressively different points in space" (18, p. 381). This situation has frequently been reduced for experimental convenience to the case of two successive light sources at two separated points in space, and this experiment has resulted in an elaborate Greek-letter phenomenology of motion impressions (alpha, beta, gamma, delta, and phi). The results of this experiment have been reviewed elsewhere (for example, 1, ch. 15) and will not be discussed here. The fact is that when an adjacent order and a successive order of discrete stimuli are correlated, a continuous impression of an object in motion results. The main limitation seems to be that the interval between stimuli must not be too disproportionate to the separation between them. Hence stroboscopic stimulation differs from so-called "real" stimulation only in being discontinuous when the latter is continuous. The relations of order are the same in both.

Motion of a patterned surface. The speed and direction of linear motion are perceived with some accuracy when a

moving belt is presented to the eye. The same thing is true for the rotary motion of the surface of a disk. For both, there are lower thresholds for velocity and also upper thresholds for velocity when motion turns into blur. Acuity for motion is high at the periphery of the retina considering how weak it is for color and form. There occurs a negative afterimage of velocity in a stationary visual field in that part of it which has previously been stimulated by a moving belt or disk. The afterimage may be linear or rotary or it may be one of expansion or contraction if the rotating disk bore a spiral that contracted or expanded (Plateau's spiral). The perceived velocity of a moving surface tends to be constant at different distances from the eye although the retinal velocity of its image is inversely proportional to distance. Brown, however, discovered some other puzzling facts about such apparent velocities connected with the size of the frame or aperture behind which the belt moved and with the brightness of the surface (2). Another fact, which is interesting for the problem of the connection between retinal motion and eye movement, is that perceived velocity is reported to be somewhat faster when the eyes are fixated on the aperture than when they follow the moving pattern from one side to the other and back again. This has been called the Aubert-Fleischl paradox (2).

Deformation of shadows and the perception of depth. Linear and rotary motions presented to the eye by belts or disks occur in the frontal plane of the observer and are so perceived. So does the apparent expansion of a Plateau spiral, and this is also perceived as flat except for an occasional report that the afterimage suggests motion in depth. But the shadow of a rotating object observed from the other side of a translucent screen, although seen in one

sense as moving in the frontal plane, is often seen in another sense as moving in depth. There may be a compelling impression of rigid rotation as well as an impression of deformation. This effect has been called *stereokinetic*, and Wallach has recently named it the *kinetic depth effect* (19). Metzger had previously studied the phenomenon and its interpretation (15). The impression of rotation in depth is reversible, and the observation of this feature of it goes back to "Sinsteden's windmill" (1, p. 270).

Controllable complex movements. There have been a few experiments on multiple motions in the visual field, that is, of meaningless spots or shadows moving in systematically varied ways. Michotte, who used the method of a pair of rotated spirals visible through a horizontal slot in a screen, was interested in the perception of causality (16). Metzger, who projected on a translucent screen the shadows of vertical rods rotating on a horizontal turntable, was interested in the problem of the visual identity of the interpenetrating shadows (14). Johansson devised a method of superimposed slide projection in which each spot on the screen depends on a different slide and each slide can be given a controlled linear or circular motion. He was concerned with the perception of the *events* which his moving spots induced (12). Johansson also describes the other important experiments of this type. Heider and Simmel, using animated motion picture film, explored the possibilities of the *social meanings* which moving triangles and circles might evoke (10).

The Stable Environment

In contrast with the foregoing experiments in which the background of the motion, or the frame of the window in which it appears, is always visible stands a class of experiments utilizing points

of light in a completely dark room. The case of a single fixated point has been studied for a long time. Although the image remains essentially motionless on the retina of the observer and the spot may appear at first to be static, it eventually shows an "autokinetic" motion. It appears to wander in an erratic fashion, and the observer himself may become disoriented. The illusion disappears if the surfaces of the room become even slightly visible. The facts are summarized by Carr (3, pp. 314 ff.). Evidently the stimulation of a single retinal point is not sufficient to yield the impression of a stable environment. Sandstrom has recently emphasized that an observer cannot even point with his finger to a single spot of light in a dark room (17). Facts of this sort throw great doubt on any kind of theory of the "local signs" of retinal points.

When *two* points of light are presented in the dark, their separation is sensed and they appear connected. They may appear to wander as an autokinetic unit, but one never appears to move relative to the other. It might be said that each has stability relative to the other.

If one of the two point sources in the darkroom is made to move slowly, the conditions are present for what Duncker has called "induced movement" (4). The observer reports motion, but it is as likely to be carried by the physically motionless source as by the moving source. A frequent outcome is a phenomenal motion of both spots, each carrying half of the total velocity. The relative motion of the first to the second or the second to the first (or each to the other) is perceptible, but the motion with reference to the room is not. The room, after all, is invisible and the background of the spots is darkness.

An example of induced motion taken from common experience is the appearance of the moon seen through drifting

clouds. In this case the clouds provide an extended background for the moon, not just another spot of light, and the impression of the moon's motion is unequivocal. Duncker set up a similar situation and studied the apparent motion of a stationary spot of light projected on a rectangular surface that moved in pendular fashion from side to side. The relative motion of the spot within the frame was indistinguishable from "real" motion; it could be cancelled by setting up an opposite pendular motion of the spot itself (4).

Duncker also noted the occurrence of induced movement of the observer *himself*, both in the darkroom situation and, under special conditions, with illumination. This was, of course, a movement without kinesthesia, produced wholly by visual stimulation. Insofar as an observer perceives himself in visual space, his own movement, like that of visual objects, depends on the phenomenal frame of reference. The question is, what establishes this frame of reference or stable visual environment?

Movement of the Observer, Including Locomotion

A simple method of inducing by visual stimulation one kind of apparent movement of the observer's body has long been known. It consists of surrounding the head of a stationary observer with a cylindrical screen or curtain, filling his entire visual field, which can then be rotated around the head. The observer reports a perception of being rotated in the opposite direction—an instance of Duncker's "induced ego-movement." The impression may be as vivid as that obtained from being actually rotated in a Barany chair, and the only difference between the case of rotating the miniature visual room and the case of rotating the observer may be the absence of vestibular stimulation in the former and its presence in the latter. The

phenomenon is similar in principle to the "railroad train" illusion described earlier.

The analysis of motion perspective for a large portion of the visual field, also mentioned earlier (8), suggests that the impression of *forward* movement of the observer can be produced optically without any contribution from the vestibular or the muscle sense. This experiment, however, has not been performed. The closest approximation to it is an informal study based on a motion picture of the landing field ahead of an airplane during a glide (5, p. 230). Observers reported an experience of locomotion along a glide path toward a visible spot on the ground. This perception was clearly, however, an "as if" kind of experience, pictorial rather than natural. The motion picture intercepted only a part of the field of view. It is said that the panoramic motion picture (especially the "Cinerama") induces even more compelling experiences of locomotion, such as a ride in a rollercoaster.

There has been little or no research on the contribution of kinesthetic, tactual, and vestibular sensitivity to the experience of passive locomotion. Their contribution to the sense of passive rotation of the body has been studied, and something is known about their contribution to the maintaining of upright posture. How kinesthesia is connected with the *visually* aroused impression of locomotion is not known. The flier and the automobile driver have muscular kinesthesia for the *controls* of the vehicle but not for the propulsion of the body, as in walking or running.

The experience of *active* locomotion—of voluntary or guided movement by the observer—is of course a still more complex psychological problem, which will not be touched on in this report. Most of the experimental evidence about

voluntary action comes from studies of pursuit tasks, reaction time, and the like, which might be said to deal with manipulation rather than locomotion. A theory of movement with respect to a goal or destination is obviously of great importance, but we are here concerned with the cues or stimuli for movement as such. This may be justified on the grounds that the flow of actions, choices, or decisions during, for instance, an aircraft landing cannot be understood unless the flow of information is understood.

IMPLICATIONS OF THE EVIDENCE

There is plenty of evidence to indicate that visual motion is a "sensory" variable of experience. It has a kind of intensity (speed) and a kind of quality (direction). It has absolute thresholds, both lower and upper, like pitch. Acuity depends on the part of the retina stimulated, like form. It has a negative afterimage, like hue. It tends to manifest constancy, like size and shape. In the form of "pure phi" it can be abstracted from an object. But more than any sensory impression, it *fails to correspond to the physical stimulus presumed for it*. Whatever the stimulus for motion might be, it is *not* simply motion in the retinal image. This seems to imply that motion is not sensory. Before concluding, however, that phenomenal motion is not a function of stimulation, the stimulus conditions should be re-examined.

The distinction between "real" and "apparent" motion is unfortunate and has interfered with the search for the essential conditions. It should be noted that stroboscopic stimulation can yield just as psychologically "real" a motion as does continuous stimulation, if certain relations are preserved. A stroboscope and a moving object are manifestly different, but they are the *sources* of stimulation, not the stimuli, and per-

haps the latter are not so different after all. The facts of the experiments can be explained by the hypothesis that the retina responds to adjacent and successive *order*. If the orders correlate for the stroboscope and the object, the fact that the former is a discontinuous emitter may be unimportant. The two retinal images are similar in that the relations of order are the same in both; for example, right-left and before-after. The stimulus for motion, then, may be *ordinal*.

There is other evidence to suggest that the stimulus for motion is also *relational*. This means that it cannot be derived from the hypothetical "local signs" of retinal receptors. The fovea does not have a fixed value for breadth and height when stimulated by a single point of light. Moreover, as Duncker proved, the motion of one point of light on the retina is perceived relative to another point of light, not relative to the retina. The frame of reference for motion (or stability) seems to depend on the array of stimulation rather than the location of the receptors; it is transposable over the retina. Just as a motion for the physicist can be specified only in relation to a chosen coordinate system, so is a phenomenal motion relative to a phenomenal framework (13). Perceived motion occurs in a perceptually stable space or environment. Another way of saying this is to assert that the perception of stability is part and parcel of the perception of motion; you cannot have the latter without the former.

The optical stimulus conditions for a stable environment seem to be a retinal image containing many elements rather than a few or one. This can be described as a differentiated or "textured" image (7, 9). Perhaps stability goes with the perception of a surface or an array of surfaces extending over most of the field of view. The disappearance

of the autokinetic illusion when the darkroom is even slightly illuminated is consistent with this hypothesis. So is the occurrence of the moon-in-the-clouds illusion. So also is the railroad train illusion when we take the window-filling train on the next track to be motionless. Perhaps *the textural background image, whatever its relation to the anatomical retina, always tends to determine the phenomenal environment, and the more it approximates the total image the greater the stability.*³

Common experience suggests that we can perceive the motion of an object in depth as readily as its motion at right angles to the line of sight, and the experiments with deforming shadows on a translucent screen tend to bear out this suspicion. The kinetic depth effects so far obtained depend on perspective transformations of the shadows, and yield impressions of changing slant or rotation. There is no reason why they should not also be obtained with size transformations of shadows, which will yield impressions of linear approach and recession. A general hypothesis is suggested by these experiments, namely, that *any regular transformation of a bidimensional image tends to yield a tridimensional motion in perception, and the kind of motion perceived depends on the kind of transformation.* This hypothesis has the advantage of relating the experiments on moving shadows to experiments on shape constancy and size constancy, and suggests a principle of space perception that may be common to both. The fact that the transverse motions of a pair of belts observed at different distances can be judged equal in velocity when the surfaces are actually equal in velocity (if

Brown's results [2] are accepted) points in the same direction.

Facts about the perception of bodily movement as distinguished from object motion are scarce. They are enough to suggest, however, that the impression of oneself being moved, like that of an object being moved, depends on the perception of the space in which the movement occurs. Ego movement like object movement can be induced. The train illusion and the cylinder rotating around the head are examples. The perception of forward locomotion can probably be induced, and the experiment should be tried. This will require optical stimulation governed by differential angular velocities for many points in the visual field, i.e., motion perspective or, crudely speaking, an expanding image.

A promising hypothesis for research would be that *any transformation of the total retinal image, as distinguished from a part image within it, tends to yield an experience of a movement of the observer, and the kind of movement experienced depends on the kind of transformation.* For example, a simple translation of the image may contribute to the experience of an eye movement; an expansion may contribute to the experience of forward locomotion; a contraction to the experience of backward locomotion; and so forth.

There is said to be a striking lack of correspondence between the presumable optical stimuli and the ensuing visual perceptions of motion or movement. The evidence does indeed show what appear to be obvious discrepancies. It is certainly true that kinetic impressions are not *copies* of their stimuli. But it fails to follow that they are not *functions* of their stimuli. It cannot simply be assumed that a movement is the same thing in the object, the retina, the brain, and consciousness. The foregoing hypotheses make it possible to

³ This hypothesis is consistent with, if not essentially the same as, the position taken by Duncker in his admirable study of "induced" movement (4).

test for psychophysical correlations, although they do not imply any pictorial correspondence, between the dimensions of the stimulus and the qualities of kinetic experience.

HYPOTHESES ABOUT KINETIC RETINAL STIMULATION

A psychophysics of kinetic impressions would require a mathematical analysis and classification of the motions or transformations of a retinal image. This is a complex and difficult task for the future. Some preliminary assumptions are possible, however.

Geometrically, one can distinguish between a *rigid* and a *nonrigid* motion of a form or of a set of points. *Translation* and *rotation* are the types of rigid motion with which we are concerned. The figure after displacement is congruent or identical with the figure before displacement. The kinds of nonrigid motion are diverse and are still being explored by the higher branches of geometry. However, two classes exist, which may be called *elastic* motion and discontinuous or *disjunctive* motion. In the former, the lines of the geometrical form do not "break up" (or the set of points maintains the relations of neighborhood), whereas in the latter the form is ruptured (or the points are "scattered"). The class of elastic motions includes two types, the *size transformations* and *perspective transformations* on the one hand and *nonperspective transformations* on the other. The first type can be defined as a projection of the form or pattern on a plane different from its own, either an enlargement (or reduction) or a slant projection. The second type can be defined as a deformation other than these, but for which the continuity of the form is preserved. The class of disjunctive motions includes many types, which do not need to be specified here, but all involve discontinuity. The six types with

which we are concerned are tabulated below:

Rigid motion

1. Translation
2. Rotation

Elastic motion

3. Size transformation
4. Perspective transformation
5. Deformation

Disjunctive motion

6. Multiple movements

These abstract mathematical motions are interestingly related to optical stimulation. Let us assume an eye and a reflecting surface, such as the face of an object toward the eye, and let us consider the cross-section of the sheaf of light rays to the nodal point of the eye (18, pp. 326 f.). This is equivalent to the retinal image. What tridimensional events produce these motions of the bidimensional cross-section? Numbers 1 and 2 above correspond respectively to a lateral movement of the eye (or the object) and a swivel movement of the eye (or a rotation of the object). Number 3 corresponds to a movement of the eye (or object) along the line between them. Number 4 corresponds to a planetary movement of the eye around the object or an inclination of the object to the line between them. Number 5 corresponds to an event confined to the object—a fluid or elastic motion of its substance. Finally, number 6 probably corresponds to an event such as the shattering of a single object or the interaction of multiple objects. Some of these statements need qualification in order to be exact, but they may serve as preliminary general rules. In other words, some very important types of physical events correspond to the geometrical types of motion in the projection. It is a reasonable hypothesis that the eye can *register*

these geometrical types of motion when they occur in the retinal image.

It may have been noted that the physical events corresponding to motions number 1, 2, 3, and perhaps 4 are ambiguous. Whether the eye moves or the object moves, the result is the same. The optical situation assumed in the previous paragraph consisted of an eye and a single object (specifically a plane face of an object). A more typical optical situation would consist of an eye and an *environment*. Let us therefore assume instead an eye and an infinite plane surface. This is a better approximation to the terrestrial environment. Except for the "sky," the image of the surface occupies the whole of the retina, and it constitutes a textured background image rather than a delimited object image. An infinite plane surface would be physically stable and would constitute an excellent frame of reference for visual perception. There is evidence to suggest that a background image does help to determine the stable phenomenal environment. Ambiguity of perception as to whether the eye moves or the environment moves in *this* situation would therefore tend to disappear.⁴

The types of physical events producing the geometrical types of motion of a total background image are fairly univocal. Translation and rotation of this image can hardly be caused by anything but eye movements. Size and perspective transformations for the elements of an extended plane surface constitute motion perspective (8) and this can hardly be caused by anything but locomotion with respect to the surface. Certainly it is true that any eye movement in an illuminated environment

causes a rigid movement of the image, and any transportation of the eye causes an elastic movement of the image.⁵

The causes in the environment and the results in perception of deformations and disjunctive motions of the image (numbers 5 and 6 above) are complicated. So far, we have been assuming a solid environment. Nonperspective deformations are caused by liquid or fluid motions of physical objects and surfaces. Rivers flow, smoke swirls, rubber stretches, and above all living organisms flex their surfaces in many ways. The faces of men, for instance, undergo an astonishing variety of rubbery motions, which we call facial expressions. We perceive these motions, sometimes with great acuity. We do not seem to confuse them with the mechanical motions of solid objects which tilt, slant, advance, or recede with a kind of inanimate quality. There may be a basis in optical stimulation for this difference.

Disjunctive motions of the image are caused by a still greater variety of events. Objects break, ants swarm, billiard balls collide, and men shake hands. Michotte believes that multiple motions can yield immediate impressions of causation that are specific to the relations between them, and he has fortified his belief by experiments (16). The possibility of isolating high-order variables of stimulation in such images seems remote, but it should not be rejected.

In conclusion, the various motions of objects in a stable environment and the various movements of ourselves in that environment can both be visually perceived. A psychophysics of such kinetic impressions, however, is almost nonex-

⁴ If to our disembodied eye we add assumptions about gravity, posture, muscles, and kinesthetic stimulation, the ambiguity would certainly disappear. But we are here concerned only with optical stimulation, admittedly an abstraction.

⁵ The classification of the motions of a retinal image here given is considerably revised from that proposed previously by the writer (6, p. 131 ff.).

istent, and the possibility of isolating their stimuli has been doubted. If, however, the effective stimulation is taken to be ordinal and relational, it falls into several mathematical classes, which are neatly correlated with types of physical events, and which may prove to be psychophysically correlated with modes of kinetic experience.

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VARIABLES AND FUNCTIONS¹

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Psychologists use such mathematical terms as variable, independent and dependent variable, and function, as well as the mathematical symbolism for representing functional relationships. But they often fail to specify whether or not these terms and symbols have the same meanings as in mathematics, and thereby, it seems to us, pave the path for confusion. There follows an attempt to highlight some of the differences between mathematicians' and psychologists' uses of these terms and symbols.

WHAT IS A VARIABLE?

Mathematical texts generally define a variable as a symbol that may represent different objects during the course of a given discussion. The set of objects with which the variable may be identified is called the variable's range or domain. For example, if T represents the set of all integers, and if the domain of variation of X is T , then X may be considered to represent any arbitrary integer. The number of objects in the domain of a variable may be limited (e.g., the set of all numbers from 1 to 10) or unlimited (e.g., all even integers). Mathematicians have found it convenient to include a constant, a symbol restricted to represent one object during the course of a particular discussion, as a special case of a variable by thinking of a constant as a variable whose domain of variation consists of only one element.

Psychological texts do not usually state explicitly what they mean by the term *variable*. One recent report, which does

offer a definition, effectively eliminates the possibility of considering a constant as a special case of a variable. The report requires of a variable that it *vary*, that it assume at least two values, and accordingly defines a variable as "a set of two or more categories such that, if any object or event be a member of one of those categories, it may not be a member of any other of those categories" (1, p. 46). Psychologists generally have not indicated whether they would consider a constant as a case of a variable. But some have done so implicitly; for example, in speaking of "the stimulus variable," they intend reference to only *one* object, say, a lever.

There is some difference of opinion among psychologists as to the major *kinds* of variables. Some psychologists recognize two kinds of variables, stimulus variables and response variables, or, as Spence (11) describes them, S variables and R variables. Other psychologists speak of three kinds of psychological variables: stimulus, response, and organismic variables (7); or variables pertaining to behavior, to environment, and to individual differences (13). In addition, some, but not all, psychologists regard intervening constructs (or logical constructs) as another kind of variable. Still others divide variables according to whether or not they are experimentally manipulable.

It is not clear whether this division is intended to constitute a stipulation of the range of variation of a given variable. It is a serious shortcoming of psychologists' usage of the variable concept that the range of variation is usually not clearly specified. While the mathematician stipulates that the vari-

¹ This is a condensation of a chapter to appear in the authors' book dealing with a variational approach to psychology.

able X is limited to integers or to real numbers, for example, or that it may range over the entire complex number domain, and while he may explicitly eliminate certain objects in the range (e.g., all values of the variable that would introduce division by zero [10, p. 99]), the psychologist does not usually indicate what is to be included in, or excluded from, the domain of variation. Accordingly, the psychologist may speak of the stimulus variable without specifying whether its range includes all stimuli or only one stimulus or a certain restricted class of stimuli. Similarly, he may refer to the response variable as if it were permitted to range over the set of all possible responses or at least all responses made by the subject during the experimental session, and yet, in actual practice, he may identify the variable only with the time required to trace a maze or with the frequency of lever pressing within a given time, thereby effectively ruling out many "responses," e.g., those referring to the pressure on the lever or the member of the body which pressed the lever. Perhaps it would help to decrease confusion if psychological writings made a point of explicitly mentioning the domain of the variable.

There is also a difference of opinion as to whether quantification is prerequisite for a variable. Some psychologists hold to the belief that a variable should properly be "quantifiable," that it should pertain to things measurable: to numbers. Only if such quantification is a characteristic do they speak of a variable. Bergmann and Spence (2) write that when a relevant factor underlying a phenomenon *has become quantifiable*, it is called a variable. But other psychologists (e.g., 7, p. 4) claim that many of the variables used in psychology are qualitative and not quantitative.

Even those who permit qualitative

variables in psychology often express the opinion that they are less preferable than quantitative variables and that the aim should be to introduce measurements whenever possible. In short, the most desirable domain of variation in psychology would seem to be a domain of numbers. If we use the phrase "numerical variable" to refer to a variable whose domain of variation consists of numbers, then we may sum up this state of affairs by noting that psychologists currently tend to favor, or even to advocate a restriction to, numerical variables.

This is not the case in mathematics. By no means are all the variables with which the mathematician deals numerical variables (5, p. 273). He is concerned also with nonnumerical variables, with those whose domains of variation do not consist of numbers. He may be dealing with a variable that ranges over the set of all triangles in the plane, or the set of all curves in the plane joining two given points, or the set of all minor arcs on a sphere, or the set of all properties remaining invariant under a given transformation, etc. It may be retorted that some of these domains of variation consist of objects that are "quantifiable." For example, one might measure the area of a triangle or the length of a curve. But this is quite beside the point. When a variable has as its range, say, the set of triangles in the plane, then the variable is identified with a *triangle*, and not simply with any or every "quantifiable" or "numerical" aspect of it such as the area of the triangle, or its perimeter, or the heights of its altitudes, or the numerical values of its interior angles, or the lengths of its angle bisectors or medians. The range of variation is the set of triangles in the plane, and not the set of quantifiable or measurable aspects of the triangles.

Possibly the current trend in psy-

chology toward quantification and the emphasis on numerical variables stem from the prevalent belief that science typically strives to quantify its constructs (11). Whether or not this is the case, it may be a comfort to psychologists who are uneasy about "qualitative" variables to keep in mind that mathematics, as a tool of the sciences, is equipped to deal with certain qualitative relationships, and that mathematicians (including mathematical physicists) freely work with nonnumerical variables and neither regard them as inferior to numerical variables nor insist upon transforming them into the latter.

WHAT IS A FUNCTION?

If to each value of a variable X with a range M , there is associated one or more values of a variable Y with a range N (where M may or may not be identical with N), then Y is called a mathematical function of X . This is usually written symbolically as $Y = f(X)$. If only one value of Y belongs to each value of X , Y is called a single-valued function of X and, otherwise, a multiple-valued function. It may happen that to a variable Y there are associated values of a set of variables, $X_1, X_2, \dots, X_n, \dots$, with specified ranges in which case $Y = f(X_1, X_2, \dots, X_n, \dots)$.

Psychologists make frequent use of the terminology and symbolism of mathematical functions. But they sometimes use these in a manner that differs radically from mathematical usage. For example, in psychological discussions we may find the concept of function, represented by such symbolism as $R = f(S)$, where R represents response and S represents stimulus, interpreted implicitly or explicitly to mean a causal relationship, e.g., that S is the *cause* and R the *effect* of this cause. But a mathematical function does not (in

mathematics at least) denote cause and effect.

In Courant and Robbins (5), we find the following:

A mathematical function is simply a law governing the interdependence of variable quantities. It does not imply the existence of any relationship of "cause and effect" between them. Although in ordinary language the word "function" is often used with the latter connotation, we shall avoid all such philosophical interpretations. For example, Boyle's law for a gas contained in an enclosure at constant temperature states that the product of the pressure p and the volume v is a constant c (whose value in turn depends on the temperature): $pv = c$. This relation may be solved for either p or v as a function of the other variable, . . . without implying that a change in volume is the "cause" of a change in pressure any more than that the change in pressure is the "cause" of the change in volume. It is only the form of the *connection* between the two variables which is relevant to the mathematician (5, p. 276).

Psychologists have also employed the function concept to represent a prior-subsequent relationship so that $R = f(S)$ is interpreted as implying that S is prior and R subsequent to S . This has led to controversy as to whether it is the stimulus or the response which enjoys priority (cf. 1, p. 48). But a mathematical function is not a representation of prior-subsequent relationships any more than it represents cause-and-effect relationships. For example, the functional relationship between areas and radii of circles implies neither the priority of the radius nor that of the area.

Mathematicians have found it convenient to conceive of a function, $Y = f(X)$, as a mathematical operation $f()$ which, when applied to X yields Y , or as a mapping or transformation of one domain into another, in this case the domain M of the variable X into the domain N of the variable Y . It has been pointed out that mathematicians usually stress the form of the connection

between the variables, the "law of correspondence" or the operation $f()$, while physicists are usually more interested in the *result* of the operation; "confusion can sometimes be avoided only by knowing exactly whether one means the operation $f()$ which assigns to X a quantity $u = f(x)$, or the quantity u itself, which may also be considered to depend, in a quite different manner, on some other variable, z " (5, p. 277). In psychology, too, confusion can sometimes be avoided if in a given context the referent of the term *function* is indicated. For example, in dealing with $R = f(S)$, psychologists may unwittingly confound the discussion if they do not specify whether they are using *function* to designate the operation $f()$ or R itself.

Finally, we should like to refer to the current stress in psychology on explicit formulations of functional relationships, on determination of the exact mathematical formula connecting variables. This is well exemplified in the writings of Hull (8, 9) and Spence (11). Spence writes, "Instead of knowing merely that the response, R , is some function of the variables $X_1, X_2, X_3 \dots X_n$, he [the psychologist] desires to know the precise function." Hull is highly critical of unstated (nonexplicit) functional relationships and argues for explicit determination of the mathematical formula connecting the variables (8, p. 29). He contends that it is better to make an incorrect guess as to the exact nature of the relationship than to work with non-explicit relationships, and calls for "determination of what the function actually is" (9, p. 173).

This emphasis may be related to the belief that in order to characterize a mathematical function it is necessary to have an explicit statement of the exact mathematical formula connecting variables. But this involves a notion of

function and of functional relationship that has proven *too narrow* for the needs of higher mathematics.

To Leibniz (1646-1716), who first used the word "function," and to the mathematicians of the eighteenth century, the idea of a functional relationship was more or less identified with the existence of a simple mathematical formula expressing the exact nature of the relationship. This concept proved too narrow for the requirement of mathematical physics, and the idea of a function . . . was subjected to a long process of generalization and clarification (5, p. 273).

Note that the first impetus for a broader conception of the idea of a function came from mathematical physics, that example par excellence of the fusion of problems of a science with mathematical methods. If behavior scientists are to be successful in evolving a "mathematical psychology," it may be necessary that they also discard a too narrow view of the idea of a functional relationship. In any event, the generalization of the idea of function is by no means confined to mathematical physics or to physics but is common to all of higher mathematics. Often the mathematician is concerned with functional relationships for which he does not have a simple, or even not-so-simple, mathematical formula that precisely stipulates the nature of the relationship. But this need not be a handicap to further mathematical treatment since the modern conception of function does not require that it be expressed in terms of a combination of elementary functions such as logarithms, exponentials, sines, etc., or that it be expressed as a power series, as a trigonometrical series, or in terms of derivatives or integrals. So long as to each value of the independent variable or to the set of independent variables there corresponds a value of the dependent variable, then the latter is said to be a function of the former despite the lack of any more precise

mathematical formulation. The mathematician may therefore prove (or assume) that there exists a functional relationship, which he may express in the general form, $u = u(X_1, X_2, \dots, X_n)$, that the variables have certain ranges, and that certain properties are possessed by the function; for example, that it is differentiable or has derivatives up to a certain order, or that it is continuous or semicontinuous. And yet from such knowledge (or assumptions) he may be able to derive many further mathematical properties and relations. Not being able to express the exact nature of the relationship by a simple mathematical formula is therefore not a barrier to mathematical treatment, either in mathematics itself, or in physics, or in other applications of mathematics.

Likewise, ignorance of the exact nature of the mathematical formula connecting psychological variables need not prove an insuperable obstacle to further mathematical treatment. Hull suggests that the psychologist should be prepared to think in terms of higher mathematics (8, p. 400). But then he must be prepared to accept the broader notion of function common to higher mathematics, and not to be uneasy about "unstated" functional relationships if by this he means that the explicit formula is not revealed. To think mainly in terms of explicit formulas, to regard their determination as the proper aim of the psychologist (even to the extent that an incorrect guess as to the specific nature of the relationship is regarded as scientifically sounder than working with a nonexplicit relationship), may be the consequences of a narrow conception of function. To insist on this conception is to bind psychology in the swaddling clothes kicked off by mathematical physics and other branches of higher mathematics during their infancy.

INDEPENDENT AND DEPENDENT VARIABLES

If $Y = f(X)$, it is conventional to speak of Y as the dependent variable and X as the independent variable since the value of Y is dependent on the particular value of X chosen. The number of independent variables may be one, as in $Y = f(X)$; or two, as in $Y = f(X, Z)$; or n , as in $Y = f(X_1, X_2, \dots, X_n)$; or infinite, as in $Y = f(X_1, X_2, \dots)$.

It is worthwhile emphasizing that the designation of one variable as the dependent variable, and another as the independent variable, may be nothing more than a convention. There may be nothing inherent in the nature of the variables that makes one dependent on the other and not vice versa. For example, in the case of a single-valued function of one variable, $Y = f(X)$, there always exists the unique *inverse function*, $X = g(Y)$. If $f(\)$ is the function which maps a domain M into a domain N , then the inverse function $g(\)$ is the one which maps N into M . For the $X = g(Y)$, convention decrees that X be called the dependent variable and Y the independent one, thus reversing the labels attached to these variables when $Y = f(X)$. Hence whether Y or X is labeled as the independent variable may depend solely on the manner in which the functional relationship is written.

The nomenclature, dependent and independent variable, in mathematics is therefore not construed to mean that one variable is completely independent of the other. The very fact that the nomenclature presupposes a functional relation means that the so-called dependent and independent variables are actually interdependent. The function concept, we have seen, is a "law governing the interdependence of variable quantities," and this interdependence of

course works both ways. Any change in the dependent variable involves a change in the independent variable, and the other way around, precisely because the two are *connected* by a functional relation. For example, a change in the area of a circle is concomitant with a change in the radius, and vice versa.

Thus the word *independent* in this context has a meaning that is different from that associated with the word in everyday parlance. It is also different from the meaning associated with the term when one refers to a system with n degrees of freedom as having n independent variables or n independent coordinates. These n variables (or coordinates or degrees of freedom) are actually independent of one another. Attempts to describe the system by means of fewer, say $n - 1$, variables, would prove inadequate; and if $n + 1$ variables were introduced, one variable would prove to be a linear combination of the others (to be linearly dependent on the others). If a system has n independent variables (n degrees of freedom), a change in one variable does not presuppose a change in the other variables. Indeed, it is precisely the ability of the n coordinates to vary independently of one another which leads to their being described as degrees of freedom and as independent coordinates. But, we reiterate, it is not in this sense that the term *independent* is to be interpreted when discussing the independent and dependent variables of a functional relationship.

Perhaps the nomenclature of *dependent* and *independent* in the latter context represents an unfortunate choice, particularly since it seems to have produced some misinterpretations among psychologists. At least, the misinterpretation seems to be sufficiently widespread to lead Bakan to state in a recent report:

What is generally meant by the assertion of the independence of the stimulus is that it is independent of the response. The paradigm is that the response is dependent on the stimulus, but the stimulus is independent of the response. The formulation of this paradigm is $R = f(S)$ (1, p. 48).

Bakan himself is opposed to this interpretation. Whether the interpretation is as common as Bakan claims or is limited to only a few psychologists, there would seem to be a need to re-evaluate what is meant by the functional notation relating S and R , and by their "independence" and "dependence," respectively.

Presumably $R = f(S)$ is intended to convey the idea that R is a mathematical function of S . At least, the notation is that of the mathematical function, and most psychological texts imply that such a function is involved. But if the stimulus and response are related by a mathematical function—by any law, rule, or correspondence that associates with each value of the stimulus one or more values of the response—it follows that the stimulus and response are interdependent and that one cannot properly speak of one as independent of the other. If the paradigm is actually intended to be that the stimulus is independent of the response but the response dependent on the stimulus, then it would perhaps be best not to use the notation of a mathematical function.

We are not objecting to the description of the stimulus as the independent variable and of the response as the dependent variable since it is conventional to attach the labels in this manner when the notation $R = f(S)$ is used. But we are taking issue with the interpretations of the descriptive adjectives.

Bakan considers it inappropriate to talk of the independent stimulus variable because "the stimulus does not exist as a stimulus except by virtue of the responses of the organism" (1, p. 48).

Interestingly enough, the same issue of the journal containing Bakan's report also includes another article, which calls for concepts immediately physical or reducible to physical concepts, and which maintains that under this canon a stimulus "would have to be something which an experimenter could ascertain without there being any organism for it to work on" (6, p. 10); that is, a stimulus would exist when there was no organism to respond to it. While Bakan does not deal specifically with this contradiction to his own point of view, he holds that it is because of a mistaken notion of the priority of the stimulus that some psychologists speak erroneously of the independence of the stimulus and the dependence of the response variable. On the other hand, he continues, some of the very persons who talk of the independent stimulus variable (e.g., 2, 4, 12) give priority not to the stimulus, but to the response. Thus he notes that Stevens (12) accepts the priority of the operation but goes on further to specify the nature of the operation as being *discrimination*, a response.

Another possible source of the interpretation that some psychologists give to independent and dependent variables may be the belief that the former is the cause and the latter the effect, that the stimulus, for example, is the cause and the response its effect or, more generally, that the independent variables are the "initiating causes of behavior" and the dependent variables the resulting behavior.

Psychologists are, of course, free to assign priority to the stimulus or to the response or to assume a causal relationship connecting them. But then the use of the mathematical function notation may be misleading since, as we have already indicated, this notation (at least in mathematics) does not imply a prior-subsequent or cause-effect relationship.

SUMMARY

While psychologists use such mathematical terminology as *variable*, *independent* and *dependent variable*, and *function*, as well as the mathematical symbolism for representing functional relationships, they often fail to specify whether or not the terms and symbolism are to be interpreted as in mathematics. Other interpretations are sometimes implicit or explicit in psychological writings. The present report outlines some of the differences between mathematicians' and psychologists' use of these terms and symbols.

1. The term *variable* is generally left undefined in psychological texts. Whether or not a constant is accepted as a special case of a variable (as it may be in mathematics) is usually not indicated.

2. Psychologists often speak of a variable (e.g., stimulus variable or response variable) without indicating what is to be included in, or excluded from, its range of variation, that is, without indicating the set of objects with which the variable may be identified.

3. Some psychologists consider quantification or measurement essential to (or at least preferable for) a variable, suggesting that the only (or the preferred) range of variation should consist of numbers. But mathematicians work with both numerical and non-numerical variables (those whose domain of variation does not consist of numbers) and do not consider the latter inferior to the former or as requiring reduction to numerical variables.

4. The mathematical function notation is sometimes employed by psychologists to imply that one variable is prior to another in time or that one variable is the cause and another the effect. In mathematics the term *func-*

tion does not imply either a prior-subsequent or a cause-effect relationship.

5. Confusion may be avoided if psychologists indicate whether, in a specific context, they mean by the term *function* the mathematical operation $f()$ (which may also be interpreted as a mapping or transformation) or the outcome of applying $f()$ to a variable or to a set of variables, that is, whether, for $Y = f(X)$, the referent of the term *function* is $f()$ or Y .

6. We pointed out that the so-called independent and dependent variables of a functional relationship are actually mutually interdependent (since they are related by a function concept), so that it is erroneous to assume, as some psychologists apparently have, that one of the variables is independent of the other, but not the reverse. Whether a variable is designated as dependent or independent may be a consequence of the manner in which the functional relationship happens to be written, and may not be an indication of anything inherent in the nature of the variable.

7. Different interpretations of the relation $R = f(S)$, where S represents stimulus and R response, were analyzed, and the basis of current controversies was found to lie in the conflicting interpretations given to the terms *function*, *independent variable*, and *dependent variable*.

8. It was suggested that the current stress in psychology on explicit formulation of functional relationships, on determination of the exact formula or

equation, may involve a notion of function that has proven too narrow for the needs of higher mathematics, and that may be too narrow for psychology.

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BEHAVIOR UNDER STRESS: A NEUROPHYSIOLOGICAL HYPOTHESIS¹

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Stress and its effects on behavior is a subject of which both clinicians and experimentalists have become increasingly aware within the last two or three decades. In human beings it has been observed mainly under naturally occurring conditions, as instanced by the literature on psychiatric war casualties (e.g., 18) and, in the case of infants, by the experience of prolonged separation from the mother, which Bowlby (4) has found to have marked pathological effects on development. In animals stress has been studied chiefly in the laboratory, where a variety of techniques has been evolved to disrupt existing adaptation patterns and to replace these by certain forms of nonadjustive behavior. Probably the fullest and most systematic set of data derives from those studies which have been grouped together under the title of "experimental neurosis," and it is to this work that the present paper will primarily refer.

Our knowledge of the behavioral data and of many of the antecedent conditions important in this type of stress situation is now, thanks to a considerable number of descriptive studies, fairly adequate, but no completely successful attempt has yet been made to fit a theoretical framework to the re-

sults of this work. It is the aim of this paper to propose such a framework, and it is hoped that, with the help of certain neurophysiological notions, it will thereby become possible to order the behavioral data and to reach some understanding of the dynamics underlying behavior under stress.

STRESS AS DISRUPTION OF THE ORGANISM-ENVIRONMENT RELATIONSHIP

A stressful situation may be regarded as essentially one in which a major disruption of the relationship between an organism and its environment has taken place. Under nonstressful conditions this relationship tends to be relatively harmonious; the environment, on the one hand, will gratify the needs and expectations of the individual, while the organism, on the other, can adequately meet the demands made upon it by external stimulation. The relationship will suffer some disruption when the organism meets a novel situation for which it has no adjustive response readily available and in which it cannot find such a response until a period of trial-and-error behavior (problem solving) has taken place; but even in such circumstances, the disruption will generally be a minor one and adjustment will eventually occur. It is only when the difficulty of the task confronting the organism is so increased as to approach a no-solution situation that one may speak of a stress situation.

But the relation between the complexity of a problem and the capacities of the individual to solve it is only one factor defining the organism-environment

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ment relationship. Another is the degree of motivation which impels the organism to face the problem, and which thus restrains it within the situation. Only when a drive has been activated can we talk of a psychological relationship between the individual and his environment, and only when this drive is sufficiently strong to persist is it possible for a major disruption in this relationship to occur. Thus, as Lazarus, Deese, and Osler (20) have pointed out, stress cannot be defined in terms of the environment alone; the motives and capacities of the organism and their interaction with the environment must also be taken into account. Fuller's (11) definition of a stressful situation as one in which adjustment is difficult or impossible but in which motivation is very strong satisfactorily complies with this demand.

Disruption of the organism-environment relationship can be brought about in several ways, and it may be convenient to distinguish the three types of situation. The first occurs when the organism is overwhelmed by an external stimulus for which it has no adequate adjustive response available and from which it has no means of immediately escaping; this may happen, for instance, in the case of a stimulus too intense for the capacities of the organism, such as electric shock; such situations may be characterized as *traumatic*. Second, in a *frustration* situation the adequate object for an aroused drive or expectation essential to the motivational structure of the organism is not forthcoming from the environment; the result is that, instead of extinction of drive-instigated behavior, there is continued internal stimulation which the organism cannot reduce. Third, there are situations of *conflict*, where the simultaneous arousal of two equally strong drives giving rise to mutually incompatible behavior tendencies makes

it impossible for the organism to make effective use of what the environment does offer.

From the point of view of the disruption that occurs in the organism-environment relationship, it is important to note that stress situations are not sharply differentiated from non-stressful situations. But whereas minor disruptions leave no aberrant symptoms behind, and on the contrary serve the organism by calling into play new adjustive responses, there is a point in the continuum beyond which this ceases to be true; beyond this point the responses evoked begin to show those nonadaptive characteristics that are found whenever an organism is caught between its urge to find a means of adjusting and its complete inability to do so. We shall now turn to an examination of these characteristics before attempting to reach an understanding of the mechanisms responsible for their appearance.

CHARACTERISTICS OF BEHAVIOR UNDER STRESS

The behavioral symptoms which emerge under stress refer, first, to changes in general activity and, second, to changes in the learning process.

Changes in general activity can be classified under two dimensions of behavior: the *rate* and the *range* of activity. The former refers to the excitatory-inhibitory classification which Pavlov proposed and which entails the shift of activity toward either one of the two extremes of this dimension when stress is applied. In the excitatory type, which tends to be more common, behavior is greatly speeded up and disorganized, and the fine adjustment of reaction to stimuli found under normal circumstances is lost. Muscular tremor, vocalization, disturbances in respiration and pulse, mydriasis, diminished control over micturition and defecation, changes in blood pressure,

the loss of previously established conditioned reflexes, and a sensitization phenomenon in which the animal is "triggered off" by the least stimulus—all these are symptoms of the excitatory type. In the inhibitory type, on the other hand, disorganization of behavior assumes a different form, for here the shift of activity is toward the other extreme, and a decreased degree of sensitivity and a slowing down of motor responses are accordingly found. Activity may even cease altogether for long periods, and a cataleptic-like immobility manifests itself in the animal.

The second dimension of behavior with which we are here concerned, the range of activity, refers to the general constriction of functioning which occurs in a stress situation. An organism not under stress will show a fairly wide and varied range of activity in a problem situation before such trial-and-error behavior becomes narrowed down and directed toward the set of responses most likely to prove adaptive. Under stress, however, no such directed variability is apparent, and behavior, as Hamilton and Krechevsky (16) have shown experimentally, tends to lose its plasticity and instead assumes a marked stereotypy. Failure to benefit from previous experience and the tendency to persist within a narrow range of responses are thus further characteristics of behavior under stress.

Turning to the learning process under stress, we find that there are certain respects in which this process differs from the kind of learning found in nonstressful situations. These differences manifest themselves (a) in the increased rate of acquisition of certain responses, (b) in their persistence in a stereotyped form for long periods without reinforcement, and (c) in their unadaptive character. In fact, all three of these characteristics may be said to be aspects of the same feature, namely, the greatly

increased sensitivity of the learning mechanism operative under stress, which fixates whatever response is dominant at the time and prevents its being extinguished even when it is followed by nothing but unfavorable consequences. The ease of conditioning of certain pathological responses to stimuli associated with the stress experience has been commented on, *inter alia*, by Gantt (12) and Lichtenstein (21); and the stability of such responses and their persistence as stereotyped fixations have been noted in nearly all experimental studies on stress.

The third characteristic of the learning process operative under stress, namely, the unadaptive nature of the responses that are fixated, is shown in the elimination of the trial-and-error behavior usually found in problem-solving situations. Instead of a search taking place for the response that would be most effective in adjusting to the situation, it appears that whatever behavior pattern happens to be dominant at the time stress is applied will become fixated. For instance, Ullman (33), by giving shock to rats at the moment of feeding, was able to produce a compulsive eating symptom in the animals; Wolpe (36) noticed that a cat which happened to be micturating at the time when it was put under stress henceforth always micturated the moment it was again put into the stress environment; and Brown and Jacobs (6) found in the case of rats to whom shock was administered that "fear acts to intensify whatever response is dominant at the moment." Thus, the response pattern that becomes fixated is not necessarily the most efficient that the animal has available at the time of fixation: some of Masserman's (27) cats, for example, chose a response which exposed them to the noxious stimulus (an air blast) rather than one which protected them from it. Only in those cases where

stress is introduced more gradually, where its application does not take such a severe and sudden form that the disruption of the organism-environment relationship immediately becomes relatively complete, will the organism first show directed trial-and-error behavior aimed at relieving its plight. But when prolonged inability to bring about adjustment causes the aroused excitation to continue mounting, the disruption of the relationship with the environment will eventually become so great that this problem-solving situation gives way to a stress situation; thereupon the same stereotyped, highly fixative characteristics that we have already noted to be typical of stress will overtake the learning function.

It appears, then, that the learning mechanism operative in stress situations will "freeze" whatever behavior pattern is dominant at the time. Thus the door is opened to all sorts of "irrational" and ill-adapted responses, which are retained in a highly stereotyped form, despite lack of reinforcement from the environment, for much longer periods than are the more variable responses acquired in nonstressful situations.

A NEUROPHYSIOLOGICAL HYPOTHESIS

When seeking an explanation for all these characteristics of behavior under stress, we find not only that few attempts have been made in this direction but that none of the proposals put forward so far has attempted to explain both the changes in general activity and those in the learning process. Pavlov (31) sought to account for the former by his theory of the respective dominance of excitatory or inhibitory cortical processes, but in the absence of empirical confirmation this theory has now been abandoned. More detailed attention has been given to the learning process and the problem of abnor-

mal fixations; in particular, Mowrer's (29) anxiety-reduction theory has been applied to these phenomena by several authors (e.g., 6, 21, 33). An attempt is made by these writers, with the help of the concept of secondary reinforcement, to force learning under stress into the same conceptual framework as learning under normal conditions, and thus to account for both in the light of the law of effect. Eglash (10) has already effectively criticized this attempt; briefly, such a theory has yet to explain why, in the first place, the dominant rather than the most effective response is adopted under stress, often without a preliminary trial-and-error period; and secondly why this response, unlike other drive-reducing responses, assumes such a rigidly stereotyped, unvarying form, which will not even be altered when the animal perceives that the original stress situation is losing its noxious quality. Maier (24), on the other hand, makes no attempt to apply the same laws of learning to "frustration-instigated" responses as to problem-solving behavior; but he does not go beyond stating the distinction, and thus fails to point out a mechanism to account for the peculiarities of learning under stress.

In view of the inadequacy of existing theories, we are forced to look for an alternative explanation, and this, it is here suggested, we may find by turning to the neurophysiological background of the phenomena we have been discussing, with particular reference to the functional relationship of the cortex to the lower cerebral centers. It is generally agreed that in the mature animal under nonstressful conditions this relationship is mainly one of dominance of the cortex over the more primitive centers, so that the activity of the organism is on the whole cortically influenced and modified. Under conditions calling forth emotional behavior, however, mechanisms known to be subcortically situ-

ated become activated, and it appears that then the relationship changes and a shift in emphasis occurs from cortex to subcortical centers; or, as Darrow (9) has put it, whenever the cortex is no longer able to deal with afferent excitation, a process of *relative functional decortication* takes place. It may be that the physiological activity of the cortex then becomes inhibited by disruptive discharges from subcortical areas, with the result that the cortex can no longer exert its controlling influence over lower centers. Now this notion, when systematically extended to the whole area of behavior under stress, may well serve as an explanatory scheme for the peculiarities characterizing such behavior. We therefore propose the hypothesis that both in general activity and in learning under stress we are confronted with functions that must be seen primarily in relation to subcortical rather than cortical processes; that stress in fact brings about a shift in dominance from cortical to subcortical centers; and that we must therefore expect differences in behavior corresponding to this change in control. The pathological state resulting from stress may then be regarded as a chronic disturbance in the relationship of the cerebral centers, and the symptoms of this state as a function of such a disturbance.

This hypothesis is made plausible by the existence of certain similarities between the behavior of decorticate preparations and that of intact animals under stress; we shall now turn to these similarities in the belief that they will show the hypothesis to be capable of providing an explanatory scheme for the behavioral phenomena with which we are concerned. The precise neural mechanisms by which a shift in balance between cortex and subcortical centers may be brought about under stress is as yet not clearly understood, and in any case will not concern us here.

SUBCORTICAL CONTROL OF GENERAL ACTIVITY

It has long been known that decorticate preparations, in which the forebrain has been removed back to the level of the hypothalamus, will manifest certain marked changes in behavior related mainly to a considerable lowering of the emotional threshold. But it is only recently that the cruder methods of ablation have been replaced by more accurately placed lesions, so that, as in the work of Bard and Mountcastle (2) and of Spiegel, Miller, and Oppenheimer (32), the neural mechanisms necessary to the expression and suppression of "sham rage" can be somewhat more narrowly defined. It appears from these studies that the typical decorticate pattern of behavior will not be brought about as long as the rhinencephalon is left intact, but that once lesions impinge upon this structure, and particularly upon the amygdaloid nuclei, interrupting the tracts from the archicortex to the posterior hypothalamus, the behavior of the organism changes and various symptoms, presumably release phenomena, will manifest themselves. Thus, the degree of emotional sensitivity to stimulation of the operated animal is greatly heightened; even the slightest stimulus will elicit all the signs of violent excitation: biting, trembling, struggling, vocalization, piloerection, mydriasis, high pulse rate and blood pressure, and disturbance in respiration. Similarly, the decreased control over micturition and defecation appears to be due to the release of these functions from higher cerebral control. If in addition the neocortex is affected by lesions, a stereotyped, crude, and constricted form of behavior appears instead of the directed variability of activity shown by normal animals. The work of Klüver and Bucy (19) on monkeys appears to contradict these find-

ings, for similar lesions brought about markedly placid behavior; but, as Lindsley (22) suggests, the difference may lie in the fact that the temporal lobes received somewhat different treatment, or it may be that the inhibitory and excitatory functions which in carnivores lie in the rhinencephalon have migrated farther rostrally in primates. All the symptoms denoting released emotional excitability are, as we have seen, also typical of the changes in the rate and range of activity shown by animals of the excitatory type under stress, and a comparison of these two classes of behavior suggests indeed that they may both stem from the same neurophysiological basis. This would support our hypothesis that under stress the organism is no longer functioning under the same neural control as in a nonstressful situation, but rather that activity is predominantly controlled at subcortical levels.

There is, however, one important difference between the two conditions: emotional after-discharge is shown by organisms under stress but not by the operated animals. Whereas the latter cease their excitement the moment the arousing stimulus is removed, animals that have been under stress may continue in an agitated state for long periods after the experience. It has been suggested by various writers that such self-sustained activity is due to the establishment of thalamocortical reverberating circuits; if this is the case, it would show that, even though much of behavior under stress can be understood as being due to a restriction of cortical functioning and a release of subcortical centers, the cortex is nevertheless continuing to play a part, namely, in the maintenance and support of the aroused excitation.

When we come to consider the inhibitory type of reaction, which, as we have seen, is also shown in some cases under

stress, the comparison with the decorticate pattern breaks down. It is likely that we are here confronted with a different kind of disturbance of the relationship of the cortex to the lower cerebral centers, but so far no evidence has come to hand which enables us to gain more precise insight into the nature of this disturbance.

LEARNING AT SUBCORTICAL LEVELS

We have seen that the learning process under stress tends to have characteristics that are different from those operating under nonstressful conditions. If the hypothesis is to be borne out that under stress lower centers take over the control of behavior, we are required to find the mechanism responsible for these characteristics at a neural level below that of the cortex. Is there any evidence to suggest the existence of such a mechanism?

At one time it was thought that learning is an exclusively cortical function, and Pavlov (31), in particular, vigorously opposed all suggestions that the neural locus of conditioning could be subcortical. This view, however, has since been challenged. Though our knowledge of subcortical learning is still meager, several sources of evidence indicate not only that learning is in fact possible subcortically but also that for a certain form of learning the sensitivity of the subcortical mechanisms, when freed from higher inhibition, is very much greater than that of cortical learning mechanisms.

Our first line of evidence comes from the work of Culler and Mettler (8), who have shown that conditioning to shock normally entails two stages. In the first stage a generalized and diffuse pattern of skeletal behavior becomes evident, while in the second a more precise and adaptive response is selected. The first stage (which is the duplicate of the unconditioned response

to shock) occurs within a surprisingly short time; the second stage is more gradual and results in the emergence of a localized, precisely adapted response, which is adopted by the animal as the most economic and efficient way of solving the particular problem confronting it. Culler and Mettler have found that the decorticate animal is capable of the first but not of the second stage; that is, it is just as speedily conditionable in a random, diffuse way as intact animals but is incapable of adopting a precise and efficient response. This suggests that the first stage utilizes subcortical learning mechanisms whereas the second requires cortical mechanisms. (It is this later stage which Pavlov had in mind when he wrote of the difficulty of obtaining conditioning at subcortical levels.) It must therefore be concluded that subcortical centers are highly sensitive to "simple diffuse conditioning (direct discharge of substitute impulses into existing action-systems)" (8, p. 301), but that selection of the most adaptive response to fit the problem can only take place when cortical processes are operating.

These conclusions are confirmed by experiments on conditioning under curare and erythroidine—drugs which have been said to suppress cortical activity and thus enable subcortical processes to function independently. Girden (15) has clearly shown that under such conditions "the rapid development and prolonged retention of the drug-state CR" are particularly striking, and that this applies to both skeletal and blood pressure CR's. Once again the learning that is observed proceeds along simple associative lines and does not represent the more refined, problem-solving type of learning.

However, in a further investigation Bromiley (5) obtained different results; his decorticate dog showed the usual

type of learning seen in intact animals. Also using an instrumental conditioning procedure, Bromiley found the animal to be capable of giving adaptive responses which, according to the acquisition scores, were not learned at a notably speedy rate. Just what factor accounts for this difference in findings remains at present a puzzle, though one possibility lies in the fact that Bromiley's operation was almost wholly confined to the neocortex, whereas Culler and Mettler also brought about considerable destruction in the archicortex. Only much needed further research in this area will, however, give us the ultimate explanation.

But in the meantime we may note that other data on subcortical learning do support our hypothesis. Zélény and Kadykov (37) have reported their findings on the learning function of a dog after cortical extirpation. A conditioned response established to a certain tone appeared also to neighboring tones; despite 300 presentations of one of these latter tones without reinforcement, extinction failed to occur, though there was some discrimination in the form of a weakening of the response without affecting the strength of the CR to the original tone. These authors also noted the speed with which an olfactory CR developed in their dog, as only seven combinations were necessary for its acquisition.

Another source of evidence on subcortical learning comes from the work of Gantt (12) on the cardiac rate in normal animals. Gantt found that the cardiac component of a conditioned response is a much more sensitive index of learning than are the skeletal components of the reaction, that the former is more quickly formed and persists much longer than the latter. Even two years after the secretory and skeletal parts of a food response had become extinguished, the cardiac component

was still retained. A similar finding was made by Anderson and Parmenter (1), who noticed that a sheep, which had been conditioned first to a 10-second and then to a 20-second interval, persisted in showing cardiac acceleration every 10 seconds long after the motor response had been retrained to the longer interval. This phenomenon, Gantt has pointed out, reveals that a basic dysfunction may exist in a normal organism, "a kind of cleavage between the outer specific muscular and the inner cardiac expression" (13, p. 50). The subcortically controlled cardiac reaction is fixated more readily and can persist without reinforcement much longer than the cortically controlled specific skeletal response.

Finally, Hebb (17) has drawn attention to a kind of learning established early in infancy which has all the properties of a reflex in that it is little affected by set and is highly resistant to extinction. As an example of such responses he mentions the eyeblink to a rapidly approaching object, which has been shown to be learned in infancy and to be independent of reinforcement. From the anatomical studies of Conel (7) we may surmise that in early infancy subcortical but not cortical centers are functioning; consequently there can be little doubt that we are confronted here with yet another instance of subcortical learning.

It appears from these observations that there is some evidence suggesting that subcortical centers are indeed highly sensitive learning mechanisms, but that this applies only to simple associative learning. A number of studies, reviewed by Morgan (28), have shown that some discriminative learning involving rather simple habits is possible subcortically, but acquisition and extinction scores show that this type of situation does not yield the phenomena of sensitivity that we have found for as-

sociative learning. In the case of skeletal responses, increased sensitivity is probably a release phenomenon, which is suppressed when the cortex is functioning and comes into evidence only as the result of cortical ablation. The cardiac rate, on the other hand, appears to be sensitive even in the presence of the cortex and is therefore presumably not subject to cortical inhibition to the same degree as skeletal responses—a conclusion to which Gellhorn (14) has also given his support.

Whether or not the release phenomenon which we have noted can be brought about by lesions identical to those releasing general activity remains to be tested. Experiments on subcortical learning following partial decortication have so far yielded diverse results. Wing and Smith (35) found extinction of a CR to light to proceed much more slowly after the removal of the striate area than before, but in a subsequent publication Wing (34) has questioned the validity of this conclusion. Marquis and Hilgard (26) found that the conditioned eyelid response to light, which they showed to be subcortically acquired, failed to extinguish in monkeys whose occipital lobes had been removed; however, they also made this observation in normal monkeys but not in normal or operated dogs (25). No conclusions, either for or against our hypothesis, can therefore be drawn from this area.

On the other hand, we have seen that in those instances in which the cortex is indubitably no longer exerting an inhibitory influence on subcortical centers, the characteristics of learning under stress may also appear at a subcortical level. The evidence does therefore appear to be consistent with the view that in situations of stress the cortex ceases to be the chief controlling and integrating center, and the responses then acquired are the result of

subcortical learning. One might indeed view the cortex as essentially an organ of adaptation, which prevents the acquisition of completely nonadaptive responses and brings about extinction as soon as any response is no longer reinforced. In its absence, however, more primitive and less adaptive mechanisms come into force, and it is these mechanisms that may be said to account for the phenomena found under conditions of stress. Thus it appears that the subcortical centers, owing to their highly sensitive nature, fixate whatever pattern of behavior happens to be contiguous at the time, and that this pattern is then reactivated by all further appearances of the stressful stimulus, as well as by any previously neutral stimulus occurring in the same environment and thereafter also firmly associated with the response.

It will be seen that, although the present theory agrees with Maier's (24) frustration theory in postulating that fixations cannot be explained by the same laws of learning that govern "goal-oriented" responses, it goes further. For while Maier rightly protests against attempts to fit these pathological responses into the Procrustean bed of problem-solving behavior, he makes no attempt to explain the mechanism by which their "freezing" comes about. This mechanism, it is here suggested, can be found in the special nature of subcortical learning.

CONCLUSIONS

The functional relationship between cortex and lower cerebral centers is an area about which our knowledge is still scanty, and the experimental data with which we are provided are as yet limited. But what evidence there is suggests that in any attempt to understand behavior it is fruitful to take this neurophysiological relationship into account as an intervening variable and to relate

the changes to which it is subject to behavioral phenomena. This the present paper has attempted to do, and though the hypothesis of subcortical dominance under stress still stands in need of direct empirical testing, it does appear, as we have seen, that there are some grounds for believing this hypothesis to be capable of explaining the characteristics of behavior under stress. It is true that we must beware of taking too extreme a position in this matter, that the cortex is not in fact an isolatable unit physiologically (it may well be that the dichotomy between cortex and subcortical centers is, physiologically speaking, too crude). Also the part the cortex plays in the maintenance and elaboration of excitation subsequent to stress-induced breakdown must not be overlooked. Nevertheless, it seems likely that a relation exists between the extent of disruption of the organism-environment relationship and the relative degree of control exercised over behavior by cortical and subcortical centers. The hypothesized relation is such that whenever aroused excitation is unable to find a readily available response channel through which to discharge, cortical control becomes weakened and the more primitive action systems controlled by the diencephalon become correspondingly prominent. In problem-solving situations where disruption of the organism-environment relationship is not severe, the subcortical element is probably relatively small; it merely adds an emotional component to behavior that otherwise still bears the hallmarks of cortical variability and direction. But once the disruption becomes extreme, as under stress, the shift in dominance is likely to be more drastic, and behavior will then change accordingly. The possibility that such a change takes place is especially relevant when we seek to explain the learning process under stress, for any ap-

proach which insists, as the anxiety-reduction theory does, on fitting the same principles that govern learning in nonstressful situations to learning under stress runs into the danger of making unjustified generalizations. But if an attempt is made to link the two types of behavior to their neural bases, it seems indicated that the reflex-like quality of stress-fixated responses is a function, not of some sophisticated learning process, but of a relatively primitive neural level at which automatic and stereotyped responses are the rule.

The specialized nature of subcortical learning may well be a process that underlies also a variety of other phenomena in which for diverse reasons subcortical centers are likely to be dominant. These include the fixating instead of extinguishing effect which punishment has been found to have under certain circumstances (30); the particularly speedy and permanent type of learning found in certain critical periods of development in various species, which Lorenz (23) has termed "imprinting"; and the enduring nature of some forms of learning in infancy, when, as Bousfield and Orbison (3) have argued, the organism is still in a "precorticate" condition.

SUMMARY

1. A stressful situation may be described as one in which a major disruption of the relation of an organism to its environment has taken place; it is brought about when a highly motivated organism is unable to find an adjustive response to the problem confronting it. This may occur under conditions variously described as trauma, frustration, and conflict.

2. As a result of stress certain changes manifest themselves in general activity and in the learning process. Both the

rate and the range of general activity become altered, while the learning process is characterized by a highly increased degree of sensitivity, as shown by the altered rates of acquisition and extinction, and by the tendency to fixate whatever response is dominant at the time.

3. None of the theories so far advanced has proved successful in explaining the mechanisms responsible for all these changes. The hypothesis is here advanced that under stress a shift in emphasis occurs from cortical to subcortical centers, and that consequently behavior under stress must be seen primarily in relation to subcortical processes.

4. This hypothesis is supported by the similarity of general activity under stress to that of decorticate animals.

5. The hypothesis receives further support from the nature of subcortical learning; according to various lines of evidence, such learning is highly sensitive for purely associative learning. This could account for the characteristics of learning under stress, and possibly also for certain other phenomena that have been indicated.

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A NOTE ON THE CIRCULAR RESPONSE HYPOTHESIS

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Perhaps no visual aid has been more widely reproduced in psychological textbooks than a diagram by F. H. Allport which appears in his *Social Psychology* (1924). It illustrates the fact that when a child speaks his voice normally stimulates his own ears. Allport proposes that by virtue of this fact an association is formed between the hearing of a sound and the utterance of this sound by the child. This principle, usually called the circular response hypothesis, is employed to account for self-imitation (repetition of one's own acts), imitation of others, and sympathetic responses. The principle is not limited to vocal responses but applies wherever a response causes stimulation of the responding organism. The circular response hypothesis is frequently assigned a significant role in social psychology and child psychology, as well as in the introductory course. Among the recent textbooks which make use of this principle are the following: Boring, Langfeld, and Weld (7, p. 44), Dashiell (12, p. 532), Hurlock (19, p. 206), Sargent (26, p. 228), and Stagner and Karwoski (28, p. 341).

This theory is so common in psychology that authors frequently feel no responsibility for referring to its history. When an origin is mentioned, it is often credited to Allport, apparently because he was responsible for the illustration referred to above. The hypothesis is sometimes attributed to Baldwin (3, 4). Among the authors who credit Allport or Baldwin with the origin of the theory are Curti (11, p. 258), Folsom (13, p. 93), and Merry and Merry (23, p. 79).

The aim of the present paper is to call attention to the fact that the theory

is an old one. It is an heirloom derived from our associationistic heritage, redecorated to give it a modern air.

Nowadays the theory is usually stated in terms of conditioning. This fact leads to the impression that the concept arose from conditioned response theory. In fact, the earliest description of the circular response hypothesis is two centuries old. It was clearly stated by Hartley in 1749.

Hartley's application of the principle to infant speech reads as follows:

I will, in the next place, give a short account of the manner in which we learn to speak . . . Suppose now the muscles of speech to act . . . It is evident that an articulate sound, or one approaching thereto, will sometimes be produced by this conjoint action of the muscles of the trunk, larynx, tongue and lips; and that both the articulate and the inarticulate one will often recur from the recurrence of the same accidental causes. After they have recurred a sufficient number of times, the impression which these sounds, articulate and inarticulate, make upon the ear will become an associated circumstance (for the child always hears himself speak at the same time he exerts the action) sufficient to produce a repetition of them. And thus it is that children repeat the same sounds over and over again for many successions, the impression of the last sound upon the ear exciting a fresh one and so on till the organs be tired (15, p. 109).

He noted also that the same principle provides a basis for verbal imitation:

It follows, therefore, that if any of the attendants make any of the sounds familiar to the child, he will be excited by this impression, considered as an associated circumstance, to return it (15, p. 109).

A few pages further on, Hartley gave a more general account of the origin of imitation in children:

They see the actions of their own hands, and hear themselves pronounce, hence the im-

pressions made by themselves on their own eyes and ears become associated circumstances, and consequently must, in due time, excite to the repetition of the actions. Hence, like impressions made on their eyes and ears by others will have the same effect; or in other words, they will learn to imitate the actions which they see, and the sounds which they hear (p. 111).

In another place Hartley applied his doctrine of association to the origin of sympathy:

Now this in children seems to be grounded upon such associations as the appearance and idea of any kind of misery which they have experienced . . . because the connection between the adjuncts of pain and the actual infliction of it has not yet been sufficiently broken by experience as in adults (p. 487).

Hartley's immediate successors in the British association school seem not to have written concerning this hypothesis, probably because they were interested only in the association of ideas and were not interested in muscular movement. While it is difficult to make sure that one has not overlooked some statement in the extensive works of the associationists, we have been able to find references to circular reactions only in Brown (10) and in Bain (2).

The reference in Thomas Brown, whose work was first published in 1820, concerns only sympathy and is not very explicit. Brown said:

Many of the phenomena of sympathy, I have little doubt, are referable to the laws to which we have traced the common phenomena of suggestion or association. It may be considered as a necessary consequence of these very laws, that the sight of any of the common symbols of internal feeling, should recall to us the feeling itself . . . (10, p. 106).

No doubt some of the "common symbols of internal feelings" referred to by Brown are responses made by a person in distress which the person himself can sense, such as crying, shrinking, and trembling. If Brown intended to include such "symbols," then he stated

the circular hypothesis, but only with special reference to sympathy.

Bain, whose *Senses and the Intellect* first appeared in 1855, was specifically concerned with the circular response hypothesis in connection with the development of verbal imitation. Bain stated:

The sound spoken is also heard; besides the vocal exertion there is a coincident impression on the ear; an association grows up between the exertion and the sensation, and, after a sufficient time, the one is able to recall the other. The sensation, anyhow occurring, brings on the exertion; and when by some other person's repeating the syllable, the familiar sound is heard, the corresponding vocal act will follow (2, p. 415).

The wording here is reminiscent of Hartley. Bain was, of course, familiar with this writer, but possibly had overlooked or forgotten Hartley's discussion of what is here called the circular response hypothesis. Since Bain made no reference to Hartley or to anyone else in this connection, it may be that he developed this idea independently.

In surveying the history of the hypothesis under consideration, we come next to Baldwin. Prior to Baldwin the concept under discussion had received no name. Baldwin originated the term "circular response" and the term has stuck. However, "circular response" as used by Baldwin did not have the limited meaning that it has at the present time. Baldwin at times used it in its present significance, but in addition he used it to refer to other very diverse phenomena.

Among Baldwin's statements is the following: "The child who has learned to make a sound, then makes it by association whenever he hears it" (3, p. 284). Other such statements can be found in Baldwin's writings. He denied that this was an original proposal, saying: "I know that this is a widespread view" (3, p. 284). It is inter-

esting to note that whereas he stated this view, he criticized the notion that this type of association is sufficient to account for the learning of language, stating that in order to learn language the child must develop a tendency to imitate *all* sounds (3, p. 284), not merely a tendency to imitate those which are already in his repertoire.

We turn next to Stout. In discussing the child's imitation, Stout in 1903 (30, p. 82) made the brief statement that imitation presupposes a motor association between the perception of the act to be imitated and the more or less similar movements that the child has already learned to perform. "Hence, the more he has already learned to do, the more he can do in the way of imitation . . ." Stout (30, p. 158) stated that such associations are the basis for verbal imitations. He gave no reference to earlier enunciations of these views.

French (14), writing in the year following the appearance of Stout's book, indicated that he was stimulated by Stout's brief comments to work out a fuller account of imitation along the same lines. This he did in a very clear-cut manner. He explained sympathy by the same mechanism. French referred to Baldwin as well as to Stout.

We have not seen the 1913 edition of Bechterev's *Objective Psychology*, but Lewis (21) credits this book with an expression of the circular response hypothesis. The principle is clearly stated in the 1926 English edition of Bechterev's *General Principles of Human Reflexology* (5, p. 209).

H. C. Brown (9) stated the hypothesis briefly in 1916; he credited Bechterev with the idea.

By 1920, conditioning principles were well-known in America, and were widely applied as explanations in various fields. Between 1920 and 1930 several instances of the derivation of the circular response hypothesis from conditioning concepts

can be cited. Most of these seem to have been independently derived.

First among the recent rediscoverers was Humphrey (17, 18). His formal statement is as follows: "Imitative action may be defined as action involving a conditioned reflex, the secondary stimulus of which is similar to the reaction" (17, p. 5). He indicates that the child's own responses are sufficient to set up such conditioning. Humphrey was acquainted with Baldwin's concept of "circular reaction" and proceeded to show that it is not identical with the conditioning theory. This is quite true, as we have shown above, but Humphrey seems not to have noticed that although Baldwin sometimes used the term "circular response" to mean something different from what it means today, nevertheless Baldwin was acquainted with the idea that responses may become associated with their own sensory effects, and, in fact, spoke of this idea as a "wide-spread view."

The first appearance of the circular response hypothesis in an American textbook occurred in Smith and Guthrie's *General Psychology in Terms of Behavior*, which was first published in 1921. These authors wrote:

Practically all imitative behavior is made up of conditioned responses. . . . The dependence of imitation on learning is well illustrated by language acquisition . . . sounds . . . accompany the movements that produce them and, because the vowels are sustained and the consonants either sustained or repeated, these sounds also precede the movements that continue or iterate them. They thus become the conditioning stimuli for their own production, so that when uttered by others they are imitated by the baby (27, p. 132).

Smith and Guthrie did not indicate whether or not they were acquainted with presentations of this idea on the part of others.

The first German edition of Koffka's *Growth of the Mind* also appeared in

1921. Koffka referred to the circular response theory of imitation and credited it to Baldwin (20, p. 310).

McDougall, in 1923, gave a concise account (22) of vocal imitation in terms of association. Since he did so without any citation of previous writers, we are left in doubt as to whether he considered that this hypothesis was a matter of common knowledge among psychologists or whether he believed himself to be making an original proposal.

The second edition of Stern's *Psychology of Early Childhood* appeared in German in 1923 and in English in 1924. In this edition Stern (29, p. 91) described the circular response theory of imitation and ascribed it to Baldwin.

As we indicated earlier, Allport (1) expressed the concept of the circular response very completely in his *Social Psychology* published in 1924. He applied it to language acquisition, to imitation, and to sympathy. Allport states that he arrived at this theory independently, but before his book was published he came across the comparable statement by Smith and Guthrie. He does not appear to have known of other origins.

It is clear that Holt's discussion (16) of circular responses, which has attracted much attention, had an extensive historical background. Of this, Holt, the philosopher-psychologist, was certainly somewhat aware. He gave credit to earlier discussions by Baldwin (3), Bok (6), and Humphrey (18), but he did not mention Hartley, Brown, or Bain. Holt's use of this principle is much more extensive and thoroughgoing than that of any of his predecessors, and his ambitious attempt to use this and other concepts to show that all of human behavior is learned, whether it proves to be convincing or not, deserves the attention which it has received.

The foregoing account is sufficient to indicate that the circular response hypothesis of iteration, imitation, and sympathy has a long history, and that it has probably had several independent origins. It remains to be noted that it has never been subjected to an experimental test. Can it be tested? We are not sure. Perhaps, in the hands of some ingenious experimenter, it can become a testable hypothesis. But its presence in psychology is due, not to research, but to the recurrence of associationistic thought in psychology. It is interesting that an arm chair hypothesis, two hundred years old, which has never been tested nevertheless continues to find a secure place in our textbooks. Perhaps its recent affiliation with C-R theory has tended to give it respectability. It is likely, too, that it continues not only because it has been recast in C-R terms but also because no rival theory has arisen to contest its place. At any rate, it provides an interesting example of the perseverance of unsupported theory in a field which prides itself upon its empiricism.

To summarize, many authors have made use of the hypothesis that iteration, imitation, and sympathy arise because a response necessarily becomes associated with its own sensory consequences. We have shown that priority for this hypothesis belongs to Hartley, who clearly stated it in 1749. The same hypothesis seems to have been developed independently by several other writers, including Bain, Humphrey, Smith and Guthrie, McDougall, and Allport. Although this hypothesis is widely accepted, no experimental test of it seems ever to have been attempted. The facts just surveyed seem to justify the conclusions that psychologists, even eminent ones, have been poorly informed concerning their predecessors' treatments of the circular response hypothesis; and that an untested theory.

which has plausibility to support it, can still find a place in psychological textbooks.

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THE SCIENCE OF PERSONALITY: NOMOTHETIC!

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The cleavage (or perhaps cleft would be a less emotionally charged term) between the nomothetic and idiographic approaches to the study of personality is indeed, as Beck (2) has pointed out in his recent paper on this subject, "a principal and vigorously debated issue before psychology today." It follows that any serious attempt to reconcile and integrate these two opposing views should be given a sympathetic hearing, and should be attentively studied by all concerned with the concept of personality. Careful perusal of Beck's proposals, however, has brought to light what appear to this writer to be a number of fallacies that appear to make his attempt at reconciliation less appealing than it might appear at first sight.

First, let us be clear about the meaning of the words used. As is well known, they were introduced by the philosopher Windelband (14) as yet another set of terms to distinguish the *naturwissenschaftliche* (scientific, nomothetic) way of studying psychology from the *geisteswissenschaftliche* (humanistic, idiographic) manner. Allport (1) was one of the first to bring the concepts into use in Anglo-American psychology, and his exposition clearly indicates the meaning attaching to the words "nomothetic" and "idiographic." "The former [sciences] . . . seek only general laws and employ only those procedures admitted by the exact sciences. . . . The idiographic sciences, such as history, biography, and literature, on the other hand, endeavor to understand some *particular* event in nature or in society." This quotation makes it clear how difficult it would be to reconcile these two points of view; literature, even if called a "science" by

Allport (it would be interesting to know the justification for this curious appellation, probably equally repugnant to writers as to scientists), does not lie down easily with psychometrics. Beck has cut the Gordian knot by disowning "idiography" completely, and by rechristening a part of the nomothetic field "idiographic." A brief quotation from his paper will substantiate this argument.

. . . let it be noted that, so far as concerns the basic procedures of scientific method, the two methods have everything in common. They both have recourse to observation and to experiment. They analyze and resynthesize data. They draw inferences that follow the usual canons of logic, both inductive and deductive. These are the foundational approaches to scientific method (2, p. 253).

This is certainly an appealing picture, but it bears no relation to Windelband's or Allport's definition of these terms. Beck has in effect surrendered the castle of idiographic beliefs; he has given up the basic proposition that idiographic procedures are founded on the view that what he calls the "basic procedures of scientific method" are inapplicable to personality research.

Having thus emptied the term of its usual, and very useful, meaning, he invests it with an entirely new content. Quite arbitrarily, Beck divides the customary type of nomothetic research into two separate steps, one of which he calls nomothetic, the other idiographic. As far as can be deduced from his paper, it would appear that the measurement of isolated traits, such as bravery, or pride, or sense of humor, is to be regarded as nomothetic; it becomes idiographic when we "ask about any person how much bravery does he have, and

coolness, *and* pride, *and* sense of humor, *and* other variables that fuse into character" (2, p. 253). Nothing here of the complete and total rejection of such nomothetic concepts as traits, which is the main characteristic of the traditional idiographic attitude; instead, we find that when we study traits in combination, we are no longer doing nomothetic research, but idiographic! Having throughout his professional life studied traits in combination, having always paid particular attention to the ways in which they interact, modify each other, and, through their interaction, "[bring] about the total behavior which we identify as a particular personality" (2, p. 254), the present writer notes with surprise that instead of being a hard-bitten nomothetical psychologist, he has in fact always acted on idiographic principles. The reader may recall Molière's *Monsieur Jourdain*, who discovered late in life that he had always been speaking prose!

Bewilderment becomes complete when we hear that factor analysis is recommended as a favorite method of this "new look" idiography. According to Beck: "A universe of traits, variables in mutual interplay, affecting one another, these are the individual. This is the task which the idiographic method undertakes. The specific technique devised to test out the findings in this kind of universe is that associated with Stephenson—the *Q* technique" (2, p. 358). Beck is apparently referring to the method of intercorrelating persons, introduced by Thomson and Bailes (13), and factor-analyzing the resulting matrix of intercorrelations, introduced by Beebe-Center (3). (Others who have some claim to have introduced this method are Burt [4, 5], Thomson [12], and Stern [11].) This gives us a specific example to illustrate our contention that Beck's "idiography" is nothing but the old-fashioned nomo-

thetic method dressed up in slightly different clothing.

By giving preference to the method of "correlating persons" over the usual method of "correlating tests," and by implying that the former is better suited to the demands of personality research, Beck is clearly adopting the view that these two procedures give different results. It is obvious that if results of two methods are identical, or convertible into one another by some simple mathematical formula, then it is not possible to describe the one as "the specific technique" for testing hypotheses of a certain kind as contrasted with the other. Now Burt (6) and Cattell (7) have discussed this question of convertibility in detail, and there appears to be no doubt that, statistically, factors derived from the intercorrelations between persons (*Q* technique) are transposable from factors derived from intercorrelations between tests (*R* technique). As Cattell (7) points out:

The belief of some users of *Q* technique that it is fundamentally different from its transpose technique—*R*—and, indeed, a method *sui generis*, has so far been most exhaustively statistically examined and refuted by Sir Cyril Burt. . . . In the writer's experience professional statisticians take the position that there is no doubt about the transposability of factors from a double-centered score matrix though there may be doubt about the exact relation under other and special conditions. . . . *R* and *Q* techniques normally . . . (i.e. without double centering) have the completeness of their transposability slightly restricted by some inevitable mutual losses of information. The losses which then occur are (a) of the variance of the first factor (or in some conditions the first two) and (b) of the specific factors . . . (7, pp. 506-507).

The rest of Cattell's paper should be carefully studied to enable the relevance of this loss to be evaluated in relation to the question at issue; the conclusion the present writer has come to independently of Cattell's review (cf. 9) agrees completely with Cattell's assess-

ment, as well as with that of Burt, in considering the *Q* sort a very questionable procedure from the statistical point of view, which at best simply duplicates factors usually more easily and safely obtained by *R* technique. Beck nowhere answers the far-reaching criticisms made of *Q* technique, nor does he consider the identity of factors produced by analyzing a matrix or its transpose; in view of the practically unanimous verdict of those qualified to judge the statistical issues involved, we must conclude that the method favored by him produces, at best, factors also produced by the arch-nomothetic procedure of correlating tests, while at worst it is beset by so many statistical fallacies as to make results meaningless.

Beck refers to some results obtained by him with the use of this method; he says: "We have succeeded in . . . isolating six schizophrenic reaction patterns. That is, we are describing six patterns within this disease group that differ from one another" (2, p. 358). As he does not give any details, it is not possible to compare his patterns with those found along traditional lines by T. V. Moore (10), or by Wittenborn (15); given comparability of populations used, it may be predicted that there will be considerable similarity. Here again, it is difficult to see precisely what new contribution the *Q* method is supposed to make, or in what way the result is "idiographic"; method and aim alike are the stock in trade of the nomothetic psychologist. Having discussed the issues involved at length, with full experimental documentation, the writer may perhaps be allowed to refer the interested reader elsewhere (9). We may now state our conclusion. Beck has set out to reconcile and integrate the idiographic and nomothetic approaches. Instead of using these terms in their traditional sense, however, he has thrown

overboard completely the idiographic conception, and has instead rechristened part of the traditional nomothetic procedure as "idiographic." Renaming different approaches in this arbitrary fashion merely sows seeds of semantic confusion; it does not contribute to the *rapprochement* desired by Beck. The scientific and the literary views of personality are still as different and as opposed to each other as ever, and the only valid conclusion to be drawn from Beck's paper and his implicit withdrawal from the idiographic position is that suggested in the title of this article: *the science of personality must by its very nature be nomothetic*. This is the conclusion to which the writer was led after an extensive examination of the arguments and experiments adduced by many writers in this field (8), and Beck's contribution has strengthened, rather than weakened, belief in the essential correctness of this view.

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SIDESTEPS TOWARD A NONSPECIAL THEORY¹

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Occasionally man has seen himself in a mirror, and not recognizing himself, has criticized himself before he understood what he was doing.² As a result, some information has been validated in part, frequently in contradiction to the beliefs, moral and religious, of his community. While the good books have stated that man should love his neighbor and his brother, the nascent social scientists have been accumulating data concerning *what man is and what his actions are*. This eking out of information, while replete with error, has continued and appears finally to be approaching a scientific footing.

Some studies of man merely record his follies, and blandly state that the road to survival is in their removal. Treatises on social problems, social conflicts, politics, etc. are often no better. Those attempts in the study of man which will prove most useful are those which reach further and further back. Volumes which name maladies and conditions are of little help. We are certain that men are not perfect, and that terms such as neurotic personality or sick society are apt, but these namings should not lull us into feeling we un-

derstand man better.³ It is a simple statement to make: "If all men were better, this would be a better world." And yet, it is a ridiculous one. If all men were better, who knows what it would mean, and what was better? At the same time, however, these very writers do a great scientific service as they focus on certain *special* problems of developmental or of historical circumstances. Thus, Erich Fromm (2) has focused on the changes or sources of security in the transition of extended family to small family, of closed community to open community. Such a special focus aids in the understanding of some current variation, but more fundamental questions remain unanswered. Similarly, the developing theory (which is again a special theory focusing on the contemporary situation) of vertical, diagonal, and sideways mobility, while useful, leaves the important questions untouched. Again, other attempts have been made which are of a *nonspecial* nature, but these have been frequently tangential to the development of explanatory concepts; in particular, they have dealt with description of system, the establishment of frames of reference, or the specification of system-model-structural-functional frameworks.⁴

¹ Colleagues and friends have contributed in so many ways to the development of this theory that it is not possible to credit them individually. *Sidesteps* was added to the original title because it was felt that where we do not *encompass* an important area, we at least deal with it tangentially. Essentially, one cannot move *toward* a theory.

² An interesting analysis of this is to be found in Karl Mannheim's *Ideology and Utopia* (3). This is an extension of Marxian dialectic analysis applied to history and is in the area of sociology of knowledge. Other interpretations are found in the work of Robert K. Merton, Talcott Parsons, Max Scheler, and Pitirim Sorokin.

³ Texts by popular writers of psychiatric interpretations such as Karen Horney, Robert Lindner, Theodor Reik, and others tend to do this very thing.

⁴ In this connection, probably the most important attempt has been the development of the General Theory of Equilibrium. This theory states that for a given system composed of two or more elements, the average performance of the elements may be assessed. Then, it will be found that the performance of the *individual* elements may be specified as a direct *function* of their distance from the

THE THEORY OF *Deumbilification*: AN
INTEGRATION TOWARD A NON-
SPECIAL THEORY

Today, whenever one speaks of instincts, the academicians raise their noses. There is dissatisfaction with the concept, at least when applied to the human level, and this is justified in experience. An instinct is usually defined as complex unlearned behavior which arises without manifest practice, and it is evident that the study of man has shown time and time again that behaviors which were considered instinctive were not to be found in certain groups, or could easily be modified or prevented from arising. Thus, another illusion or explanation was lost; it was of no purpose to name a mystical force in man, and then explain by it. The mystical force just did not exist. However, *reflexes*, very simple automatic responses, are found. These mechanisms are studied by psychologists and physiologists, and the concept of the reflex

average performance. Further, if the direction of difference is maintained, *the sum of the differences will total to zero*. In no case will it be negative. The beauty of this theory is that *it has fitted all sets of data* to which it has been applied, irrespective of the sizes involved and irrespective of the type of distribution involved. The shortcoming of the theory is that while it is excellent for the description in the immediate, that is, the structural description, it does not take into account the sequence of structural descriptions which are the ongoing structural functional reality.

Recently, a mathematical model has been proposed for this theory. If the set of elements are called x_1 , that is, x_0, x_1, x_2 , and so on, and there are n such elements, then the expression for the sum of these can be stated as: $\sum x_i$. The average of these elements can be computed by dividing through the entire expression by n , since there are n such items. The expression then becomes: $1/n \sum x_i$. (Several steps are skipped here to simplify the expression.) A forthcoming paper will deal with this mathematical model, its extensions including the computation of the deviation from the mean, and possible applications to social science.

has withstood scrutiny. Similarly, the *drive*, the generalized and diffuse activity in the given direction, has withstood the scrutiny of academicians. The more complex and specific term, "motive," however, is often suspect, and is receiving much attention in psychology at the present time.

If we have two acceptable concepts, reflexes and drives, what can be done with them? If behavior at the complex level occurs and is not instinctive, what explanations are satisfactory? Obviously, there must be some process of organization in the organism, not only physiological but also at the manifest level which we call mental. In this field, two other concepts, *maturation* and *learning-training* are considered legitimate in terms of the logico-empirical theory. Thus, we have a concept of development for man which begins in the fertilization of the ovum; the fertile ovum is fed and nourished in the womb, developing and becoming differentiated as an organism, prepared for some stimuli with some reflexes, and prepared to alert his older fellows to his drives by these reflexes. As we conceive it, *the embryo is ready to learn as soon as it has established any behavior pattern*. The one underlying mechanism of training, *conditioning*, is phylogenetically validated. Thus, we may conceive of the embryo in the womb as *capable* of learning. *In utero*, the organism is not subject to all types of stimuli, but certain facts are well established. The infant may be felt moving by the mother in the third month, so that it is well known that it begins exercising early in life. But aside from this, from pregnancy wastage, experimenters have found that the organism may respond to various forms of stimulation long before birth.⁵ The one source of stimulation to which the organism definitely responds early in the

⁵ The literature in this area has been reviewed carefully by Carmichael (1).

womb life is tactile stimulation. We may infer, and it is the thesis that underlies this paper, that *the child learns, although the learning may be small and generalized, in the womb.*

Sigmund Freud probably has done most to explain the important facts concerning man, but even he, being a pioneer, could not drive his analysis to fruition. We do not claim to present a complete thesis here, and certainly not a panacea for curing the problems of man, but we do feel that we are definitely breaking through previous barriers and extending in a more scientific manner work started by Freud.

Any serious student will see that *all* that Freud wrote is not acceptable. What we like in Freud is his approach, his attempt to find those situations which are early and which might well have a great deal to do with the personality of people, and for that matter, of peoples. Thus, instinct aspects of Freudian theory we do not need to maintain here. Similarly, concepts which are in respects pedagogical, such as the superego, the ego, and the id, may be dismissed as superfluous in this context. However, in doing this have we dismissed Freud? We have not, in fact, for his significant contributions have to do with the development of patterns of response, and reaction to the absence of stimulation when the patterns of response are established.

Probably the most significant concept developed by Freud is that of *penis envy*. The concept is one concerning the development of an awareness of a lacking on the part of females as they learn, either through sight or indirection, that they do not have the external genitalia the males do. The awareness itself is an admission of inferiority, leading to frustration, and potentially, to the redirection of aggression which we ordinarily call conflict. This has been well developed by Freud himself, and emphasized by his

students (Adler). But what of the male? Is there a parallel situation for the male? Except for the facetious proposals of a few feminists who have stated that the male develops an awareness of lacking in not being able to *bear* a child, no serious suggestion has been brought forth. Why, then, is the male so closely associated with conflict? It is at this point that we must develop a theory which is *nonspecial*. In this, credit is due to Alfred Adler, whose emphasis on aspects of inferiority and superiority indirectly led to the discovery.

Often it is possible to overlook the obvious. Sex, as a root of problems, has been noted in the literature throughout the ages. But it took Freud to bring the obvious to the attention of the serious student! The scholar should not be blamed entirely, however, for repression and suppression are now known to account for much of this. Let us look at our last sentence once again and note the words repression and suppression. Might it not be possible that some source area of inferiority feelings is so completely repressed that we ignore it, obvious as it is? This, in fact, appears to be the case.

Freud, Rank, and quite a few others have directed attention to the prenatal period. None noticed, however, that in the characteristic position, the knee to head position, the umbilical cord of the fetus ranges and rubs against the fetus. Proportionately it is a large object, soft, but omnipresent. It may be wrapped around the fetus, or it may be caressing his face as he rotates in the womb. In any event, it is with him for the duration of his stay in the womb, and the *fetus is in constant contact with the umbilical cord*. This constant contact builds up expectation, through conditioning, of further contact. When the fetus loses the umbilical cord, an awareness of its absence is manifest. So far as is known, after parturition,

in all societies and peoples the umbilical cord is removed from the newborn, either by cutting, biting, or letting it atrophy, as it does, naturally. *It is normal course for the newborn, among all peoples, to be deumbilicated.* The absence of the umbilical cord, and the memory traces associated with it, are the underlying reasons for the insecurity manifest in man.

With this knowledge, then, many things become immediately obvious. The perpetual seeking for a better condition may quickly be associated directly with the feeling of this insecurity, the memory traces of the umbilical cord being particularly awakened under given sets of circumstances. Similarly, anxiety and insecurity may be seen as the sources underlying aggression, and the expectation of these which are built into the very nature of man in his ontogeny explains the invariant history of conflict and warfare. But why should the association be with men? Obviously the association is again one which is determined in the situational development in the physiological context. Because of the differences implicit in the existence of the external genitalia, the *additional* feeling of insecurity (or inferiority) results in the ordinary suppression of aggression, so that females tend to be aggressive only in the more devious and protected ways. The *penis envy*, thus, *serves as re-enforcement of the insecurity condition.* Similarly, the locomotive restriction of being gravid and the dependence implied during gestation predispose for the development of more devious outlets for aggression among the females. One of these forms, of course, is the creation of conflict among others, and by quite natural grouping, among men. Thus, the organization of society may be expected in most cases to dispose toward conflict among men rather than among women, and this is a fact well verified in anthropological research. Ob-

versely, males, having the external genitalia, occasionally supersede the feeling of insecurity by emphasizing the surrogate. *The penis may serve in some cases as the umbilical cord surrogate.* The memory traces, however, are not removed, and the constant presence of aggression leading to conflict and warfare is evident. The surrogate, the penis, leads to exhibitionism (as previously noted by Freud), and it is for this reason that we have the strong association among peoples of cults of beauty (almost synonymous with manhood), dancing, singing, expression in the artistic forms, and even the deities, with the male. A concept such as castration complex develops naturally with conditions associated with the positively re-enforced behavior.

We will not press the universality of the observations made here. However, consider the tremendous repression of umbilical reference in our society alone. *There is no known profanity with an umbilical reference!* No other ordinarily repressed or controlled area is this fully repressed.

On the side of symbolism, all societies are familiar, though they refuse to recognize them as such, with umbilical symbols. In many cases umbilical symbols such as the snake, the coiled snake, macaroni, and many others, have been identified erroneously as phallic symbols. A prime example is the identification of the male figure on the cover of the telephone directory as the paragon of phallicism when most obviously it is the epitome of umbilicalism.

On the side of behavior implicating memory traces and direct expression we have examples too numerous for detailed presentation. Consider the child's play with the umbilicus, or the religious example of Buddha contemplating his navel. Of particular interest is the universality of pleasure associated with caressing, and in particular, nuzzling, which are forms simulating the caressing

of the umbilical cord. Nuzzling, it should be noted, has been demonstrated to occur through the phyla, while such behavior as kissing, nose rubbing, etc. has been demonstrated to occur only in *some* human communities. Further, while sexual intercourse is universal, pleasure is not necessarily associated with it, as is seen from frigidity studies among people, and passivity studies among females in various species.

One point of this brief paper is to indicate how the development of the nonspecial theory has already eliminated an existing theory and replaced it by more accurate description. A major contribution of Otto Rank was his theory of *birth trauma*, which essentially stated that generalized insecurities might be associated with the leaving of the womb, and in the pain and filth and gore involved in the extrication. While plausible, the situation did not become clear until recently. First, the large number of Caesarian operations has bred a population largely spared the "birth trauma," but no essential difference has been reported between this group and normal births. Thus, the special theory is demonstrated to be erroneous, and evidence is produced which is consistent with the nonspecial theory. It is evident that *the loss of the umbilical cord is the trauma*.

FURTHER DEVELOPMENTS: MAMMARY ENVY

One of the most immediate reactions to the presentation of the nonspecial theory, which deals with the relationship of deumbilification to response characteristics, was the proposal that in fact the underlying source of *order* in behavior is attributable to *mammary envy*. The initiating proponent writes:

I cannot develop the whole theory [of mammary envy] in this note of thanks for your lecture, but I am sure that you can see the many implications which can flow from the recognition that male and female human beings are, at various times in their lives, and

in varying degrees, always *with* and *without* breasts. The meaning of this fact for early infancy is obvious, but it has the greater value of adding the developmental dimension to Freud. He never adequately appreciated the mature years. The unsuccessful efforts which have been made to develop the anal-oral-genital sequence may now be replaced by more fundamental analysis. Your contribution is a real breakthrough for me although closure is yet dimly perceived. I can sense, however, the enormous integrative power of a faintly conceived, pentagonal, three-dimensional paradigm ranging clockwise, and *irreversible*, from umbilicus on through anal, oral, genital to mammary.

The immediate reaction to the proposal was acceptance. This, however, did not prove fruitful. After considerable *empirical discussion* and *empirical thinking*,⁶ it was discovered that certain consistencies and inconsistencies needed to be specified and accounted for. While there are feeding differences, on which a considerable amount has been written, implications of feeding are commonly translated into love nurturance and other concepts designating affective ties. Rejection of this type of analysis became necessary.

It is at this point that the *cross-cultural approach* became most useful.⁷ Whole cultures differ in their feeding habits, and for this reason the personality complexes of individuals may be examined as a function of the feeding differences. In this way we found the clue of the relation of mammary envy to the nonspecial theory to lie in the family system of certain less mechanized cultures. Before introducing the datum from which we derived the clue we shall develop the relationship of person to breast.

First, let us acknowledge that a non-breast-fed baby may have to learn cer-

⁶ Empirical discussion and empirical thinking are forms of research. Although not usually presented in most methodology texts, these research approaches have great prominence in actual practice.

⁷ This is another example of the fertility of interdisciplinary research.

tain of the modes of response in relation to mammary envy through secondary sources, through the intellect, or possibly, through the belated stimulation of physiologically (and phylogenetically replicating) facilitated patterns.⁸

Let us then consider the breast-fed baby. Breast feeding is temporally *after* the umbilical stage, and the onset is almost concomitant with deumbilification. The child is nestled comfortably with the mother, and it has access to the nipple and the thin milk. Nestling involves the rubbing of the face of the infant on the breast, and other tactile stimulation. The hands of the infant, usually closed, however, are not involved. *The infant itself has no cognition of breasts of its own.* These are basic facts.

Among maturing girls in our society, there is some interest in the having of a reasonably ample bosom. A good point of reference in recognizing the passing into young womanhood is the development of the breast. Having the ample bosom is desirable, although the absence of it is not necessarily disastrous. In societies where breast feeding of babies is the usual thing, having an ample bosom may be one visible evidence of being able to nurture a family. Even up to recent times there has been some pride and but little shame associated with publically nursing the young. Occasionally, the having of bosom has been considered as a negative value, but never in any serious sense. We have in our history, for example, the pencil silhouette and other styles which have tended to constrict or hide the bosom for the female. These perverse conditions, however, rarely persist.

⁸ Walking, for example, is not described as instinctive. However, when the organism is matured sufficiently physiologically, learning to walk may be almost instantaneous to the first trial.

Unlike the umbilicus, there is little repression associated with the breast. The breast has been exposed in painting and sculpturing throughout history, and there is much reference to the breast in erotica. In American society of today the bosom is the constant point of attention for the more or less cheap pornography which is disseminated through Hollywood, pulp magazines, and other sources.

Thus, we see that there is a considerable importance attached to the bosom. Attention is of two kinds: first, in terms of the recognized function of nurturance of the young, and secondly, as an object of beauty and desirability. Now, we see that there can be such a thing as mammary envy between females in the naive sense that some females have and some haven't the appendages, and those who haven't may desire. However, this is not important. The important focus is in the *possessing* of breasts rather than in having them. We can immediately look at our cross-cultural picture and get the entire information required for analysis of the role the breast plays. The clue to the importance of mammary envy came when it was noticed that in terms of possessing breasts, *there are differences and these differences basically underly the familial patterns which are so varied throughout the world.* Thus, in a *polyandrous* society the female (as a daughter in the family) may be considered undesirable. However, desirability of the bosom is still evident in the fact that two (or more) men, usually brothers, may share the female. It is interesting that other animals may be highly prized in a *polyandrous* society, while, females (who may be in a category with animals) may not be so prized. *Monogamy* we consider as a more or less restrained approach, primarily associated with sophisticated, highly developed, structured society which must control its

more obvious "motivational" sources by artificial means. This is not to state that monogamy is associated only with higher societies in terms of technology and population density, because this is not the case. The *polygynous* family organization is extensive and is indicative of certain conditions. Polygyny is found throughout the world, whereas polyandry is associated primarily with areas where there is considerable deprivation in terms of the available resources in the ecology. But what is interesting about polygyny is that it occurs in those areas which are not so much known for the technical development but rather for sensitive art and control of emotion. The control may be at times posed by the society in terms of a hierarchy, or the control may be one which is associated largely with the tradition which is familial, though extensive throughout the social community. By the standards of which we speak, most of Western civilization would not pass as artistically sensitive or controlled in emotions. We find, thus, that societies which are polygynous are not those with which we are most familiar.

What the analysis of family forms led to was that *where polygyny is found, so is found the prizing of stock animals, particularly cows*. We come to recognizing the relationship. Cows tend to be worshiped and prized in the societies which are polygynous, and it is interesting that almost invariably cows and wives or females are interchangeable. More interesting is that *in many places the cow is more valuable than the wife*. This leads immediately to the difference between wives and cows, which is a *binary quantitative variable*. The principle of the possession of mammary glands was first recognized in this context. Breasts are the prized objects, and looked at cross-culturally, it can be seen that, where the *range of collec-*

tion is relatively unrestricted, cows and wives are both collected for their mammae, and cows may be preferred.

What became evident in the terms of the nonspecial theory was that while mammary envy was not operating as an underlying source of motivation, it did operate as a reinforcement of the general insecurity feelings which existed. It was found that where there was control of the collection of mammae, there was associated also a greater amount of insecurity, and this, of course, can easily be verified. We have already noted that in the polyandrous society people live at a subsistence level, and this, itself, is associated with a great deal of insecurity. Persons who possess many mammary glands are ordinarily those who have greatest security in terms of other forms of possessions as well. Even in a society such as our own we find that possession of mammary glands is to some extent associated with being a secure person. Economically, it is only the wealthy man who can afford a mistress, or even an occasional replenishing of the allowable mammae by divorce and remarriage. In terms of adjustment security, we find that mental illness is associated with being an unmarried rather than being a married person. The fact that it is a matter of possession of mammary glands rather than the having of them is attested to in that the mentally ill persons are more likely to be females than males.

Before passing on to the next important reaction and further contribution to the nonspecial theory, let us just mention in passing that symbolism of mammary envy has been neglected to some degree, even though the emphasis on mammary, such as is noted in our own society, is quite prominent. Very few people, for example, have recognized the importance of mammary envy in their desire (projected) to conquer a

mountain. Consider the attention that climbing to the peak of Everest has brought upon two recent explorers.

FURTHER DEVELOPMENTS: DIGITAL GRATIFICATION

Another form of reaction to the non-special theory pointed to developments which are frequently associated today with fallacies in observation logic. If Freudian symbolism gives everything that is done phallic connotation, this detracts from the explanatory value of the phallic symbol. We have already indicated that there may be other items that have been grossly ignored.

Probably the greatest contribution that has been made in the development of the nonspecial theory of umbilicalism is pointing to the fact that in the sequence of umbilical to mammary there is concomitant another type of development which is again located strongly in the response to the environmental situation. This is a factor of maturation development and learning-training. We have noted in presenting the material on mammary envy that the infant suckles but does not have the use of his hands. What is of considerable interest is that *the facility with the hands is the things that characterizes humans*. This has been recognized in terms of the thumb, or the enormous dexterity that the human has in comparison to other species. However, what is neglected is the fact that this is something that develops over a considerable period of time and that *dexterity is something that grows almost with intelligence*.

What is most emphatic is that once again the emphasis on phallicism has probably obscured a great discovery. Most of the things which are associated with the penis are probably equally well associated with the fingers; that is, masturbation as a prime example of genital gratification is something that

is associated with the hand. The emphasis has been so strong on the genitalia that actually there has been complete neglect of the gratification which is received through the tactual stimulation in the hands, here called *digital gratification*.

That digital gratification is a reality is something that we state rather than argue. One of the first responses of the infant child is that when he can no longer *possess* the breast, its substitute will be the thumb, or the fingers, or the hand, and he will place this in the mouth. It is not that the child gets gratification from sucking the thumb but that the child gets gratification in having his thumb in his mouth. *It is through the thumb that the child feels as well as through the mouth*.

That gratification is received through the digits is seen in the myriad ways in which caressing is manifest. Caressing may be self-directed, or it may be caressing of others. The same person may indulge in both. The common ground is that the person *caresses*, and it is through the digits that the person gets the gratification. We will not develop here the *Lenny Complex*, which was first noted in connection with John Steinbeck's classic work, *Of Mice and Men*.

Not only has genital gratification been used to mask this real relationship of digital gratification, but even oral gratification has been used or misused in this way by Freudians and neo-Freudians. We have already pointed to the fact that the infant gets gratification from his thumb rather than from his mouth. It is the object of oral attention which is desired, and not the mouth which merely serves as host. But consider that such things as nail biting and cigaret and pipe smoking are considered as oral gratification, when in fact they all involve primary usage of the hand. Consider also how few

mannerisms are associated with the use of the mouth as compared with the hands; rubbing, fingering, thumping, drumming, etc. Expressiveness, when not located in the language or the histrionics of voice change, is most assuredly associated with the hands. In some cultures hand gestures are a secondary form of language.

Just recently an associate put his finger on an important example in this area by bringing up the story of Peter and the Dike. Peter's action, usually interpreted as an example of great courage and devotion, is actually, in the light of this new theory, one of gross self-indulgence.

In closing this section, let us recapitulate the previous two sections. First of all, we have indicated that the source, from which certain "motives" and forms of action stem, appears to be associated with deumbilification. Second, we have noticed that security is associated with the degree of *possession* of breasts. Third, we have found that the major source of positive gratification is associated with the digits. We have essentially replaced the Freudian sequence of anal, oral, to genital with the more appropriate one of umbilical, mammary, to digital. We have not destroyed the Freudian concepts; rather, we have shown that they were properly isolated, but insufficiently so, and that they must operate within the nonspecial theory. The so-called anal, oral, and genital stages, thus, are seen to be but *special foci*.

FURTHER DEVELOPMENTS: REFERENCE PERSON THEORY

The most gratifying reaction to the nonspecial theory has been an entirely independent contribution, and this has stemmed from work on the reference point of response.⁹ In prior work, it

has been noted that after a given event has occurred, it is possible to go back and indicate the reference points to which persons were responding. That is, if groups are known to have different sets of values, and behavior is associated with the values, then if we know that a person is responding as though he were a member of a given group, we may infer that he will behave as though he were a member of a given group. Thus far, in the relevant analyses, most of the prediction has been backward in time. That is, the inference is that because a person has behaved in a given way, he has considered himself as a member of the particular group which may be expected to behave in the particular manner indicated, or *at least*, he has responded in the same way as a member of the particular group which may be expected to behave in the particular manner indicated. This does not mean that he has behaved as all members in the particular group behave, because the different members of the group may behave in quite different ways, and he may be behaving exactly like one of the deviant members who does not act at all like any of the other group members. (The deviant, of course, might himself be acting as though he were the member of another reference group.) This type of analysis is quite difficult in the forward prediction except in relatively simple cases. What is of particular value in the development thus far is the association of a person with a point of reference, not only for membership, but also for judgment. The behavior of a person, thus, includes his judgments, and these are a part of his reference point, and concomitantly, are relevant to reference group theory.

In our work we have been led to the reduction of the group to the diad, the simplest and smallest unit of two or

⁹ A considerable amount of work has already been done in this area (4).

more persons. However, our intensive treatment has even made it impossible to work with groups of this size, and forcibly, we have been reduced to groups of size *one*. However, we are still working with a diadic situation, but we deal with the relationship of two one-person groups.¹⁰ To prevent confusion of the diad with the situation of one-person groups, we have introduced the concept of the *person-group*, and in this connection, *reference person theory*. Once this distinction was made and we began working with this limiting case, it became evident that if we identify the reference person of ego, we may be able to predict his behavior in advance. In this connection we found that there was already, although it had never been brought to light before, direct relationship between reference person theory and the development of *self-exposition* of the asceto-physician.¹¹ It is exactly this relationship that tied the reference person theory to the nonspecial theory. As it turned out, it was not possible for

¹⁰ Small group research has received much attention recently. Although there is no work published, we have experimentation with the *no-person* group. Scores are randomly selected according to random procedure, and are given random meaning. Data collected in this way are leading to much insight in the study of unlikely events. In this research, to forestall possible criticism of generalizations derived from laboratory experiments, we are using a no-way mirror setup.

¹¹ The normal course of asceto-exposition is long, and parallels many of the patterns of psychoanalysis. The asceto-physician must in all cases be an MD to practice. He must diet, expose himself to all ailments and maladies known, and constantly reduce himself to points near death. Patients may revulse on first sight, but they then become full of pity (cf. transference). This condition prevails until there is a *hardening* on the part of the patient, and he becomes indifferent to the asceto-physician. At this point, the patient,

self-exposition to develop until there was a complete rejection of the current symbol interpretation. It was not until the cloak of importance of phallicism could be removed that the asceto-physician could acknowledge that *inadequacies for persons occur in all spheres*. In this, it is a crucial point that they have called themselves asceto-physicians instead of psychoanalysts or therapists.¹²

CONCLUSION

Since this is already an abstract of a monumental work, we do not recapitulate. We only note in closing that much research is currently focused on the kinds of theory we have presented.

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realizing how well off he is by comparison (reference person theory), and being accustomed to the deformed, demented, and degenerate asceto-physician, may face the world on his own.

¹² Asceto-physicians have refused the current name identifications pointing to the implicit acceptance of Freudian symbols in them. For example, psychoanalyst derives in three parts: psycho- analyst, one who has to do with an imaginary anus. Therapist is simply a contraction of: the rapist.

PSYCHOLOGICAL REVIEW

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YEAR	VOLUME	AVAILABLE NUMBERS					PRICE PER NUMBER	PRICE PER VOLUME
1894	1	-	2	-	4	5	\$1.50	---
1895	2	-	-	3	4	5	\$1.50	---
1896	3	-	-	-	-	-	---	---
1897	4	1	-	-	-	-	\$1.50	---
1898	5	-	-	3	-	5	\$1.50	---
1899	6	-	-	-	-	-	\$1.50	---
1900	7	1	-	-	-	-	\$1.50	---
1901	8	1	2	-	-	-	\$1.50	---
1902	9	-	2	-	-	-	\$1.50	---
1903	10	1	2	-	-	-	\$1.50	---
1904	11	1	-	-	4	5	\$1.50	---
1905	12	1	2	3	4	5	\$1.50	---
1906	13	-	-	3	4	5	\$1.50	---
1907	14	1	2	-	-	-	\$1.50	---
1908	15	-	-	-	-	-	---	---
1909	16	1	-	3	4	5	\$1.50	---
1910	17	1	2	3	-	-	\$1.50	---
1911	18	1	2	3	4	5	\$1.50	---
1912	19	1	2	3	4	5	\$1.50	\$8.00
1913	20	1	2	3	4	5	\$1.50	\$8.00
1914	21	-	2	3	4	5	\$1.50	---
1915	22	1	2	3	4	5	\$1.50	\$8.00
1916	23	1	-	-	4	5	\$1.50	---
1917	24	-	-	-	4	-	\$1.50	---
1918	25	-	2	3	4	5	\$1.50	---
1919	26	1	2	3	4	5	\$1.50	\$8.00
1920	27	1	-	-	-	-	\$1.50	---
1921	28	-	2	-	-	-	\$1.50	---
1922	29	1	-	-	4	-	\$1.50	---
1923	30	1	-	3	-	-	\$1.50	---
1924	31	1	2	3	4	5	\$1.50	\$8.00
1925	32	-	2	3	-	5	\$1.50	---
1926	33	1	-	3	4	5	\$1.50	---
1927	34	1	2	3	4	5	\$1.50	\$8.00
1928	35	1	2	3	4	5	\$1.50	\$8.00
1929	36	1	2	3	4	5	\$1.50	\$8.00
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